Air Force Technical Report AFOSR 4490

THE KNOWLEDGEABLE ANALYST:

AN APPROACH TO STRUCTURING MAN-MACHINE SYSTEMS

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AN APPROACH TO STRUCTURING MAN-MACHINE SYSTEMS

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ABSTRACT

The objectives of systems analysis are to analyze specific systems in order to solve predesignated problems. These objectives are at variance with those of the empirical sciences, which attempt to discover general laws. Since the two objectives differ, the method of systems analysis differs from the method of science. In an attempt to evolve a general method for systems analysis, the matrix-network approach for the analysis of complex man-machine systems is presented. This approach consists of seven steps which show how a system can be structured and how mathematical models of systems aspects can be incorporated into the over-all analysis. However, some of these steps involve, besides formal rules, the judgment of a knowledgeable analyst. To delve deeper into this judgment function, various logical, methodological, and psychological aspects concerning this function are discussed by different authors. On the basis of these discussions the principal author develops requirements which must be met by successful approaches to the structuring of complex systems.

PREFACE

For the past two years the Air Force Office of Scientific Research of the Office of Aerospace Research has sponsored a study by Stanford Research Institute on the structuring of complex man-machine systems under Contract AF 49(638)-1020. This is the final report on the work which was performed under this sponsorship.

The first year of the project was devoted to the development of the logic of the matrix-network approach to the analysis of complex systems and resulted in Air Force Technical Report AFOSR-2136, which contained essentially Chapters II and III of the present report. The development of this logic led me to the recognition of the importance of the concept of "the knowledgeable analyst" to systems analysis. Therefore, I asked Charles J. Erickson, Dr. John B. Fink, Dr. Maurice Rappaport, and Leonard Wainstein to assist me in exploring this concept. The results of their respective investigations are contained in the chapters they contributed to this report. As we hope to indicate by the individual authorships for each chapter, each author is responsible only for the chapter written by him, and I alone am responsible for the editorship of the entire report.

The ideas expressed by myself in this report are really an outgrowth of many discussions and working sessions on systems analysis and evaluation in which I have been involved over the past decade. In this connection special acknowledgment is due to several people. Among these are: Elmer H. Smith of the University of Michigan, who encouraged my early work in this field; Albert Shapero of Stanford Research Institute, who introduced me to the matrix presentation and the threefold classification of systems elements; and Dr. Harold Wooster of the Air Force Office of Scientific Research, who gave the impetus for the crystallization of these ideas.

We also wish to acknowledge the invaluable critical assistance which we received during this project from Dr. Paul Brock, Dr. John G. Meitner, Dr. Howard M. Vollmer, and Iva M. Warner of Stanford Research Institute, and from our Contract Monitor, Mrs. Rowena Swanson of the Air Force Office of Scientific Research.

K. H. Schaeffer

CONTENTS

CONTENTS

ILLUSTRATIONS

TABLES

x

I INTRODUCTION
by
K. H. Schaeffer

Modern technology has given us the means to assemble large complexes of men and machines, or systems, in which inputs can initiate events removed in time and space. For instance, we can pick up a telephone on the East Coast, dial a number, and thus start a chain of events which causes a phone on the West Coast to ring. Or we can drop a letter into a mailbox and expect it to be delivered in due time at a specified location. In neither of these cases is the energy used in the input the energy that produces the final event. Thus, the chain of events is not under the complete control of the initiator of the action, but is influenced by a multitude of conditions and forces. These varying influences sometimes cause the input to go astray--we may get a busy signal in our telephone call, a wrong number, or a broken connection; our letter may be delayed or missent, or even get lost. The more frequent such misadventures, the less useful the system; conversely, a system's usefulness is enhanced with increases in the reliability and efficiency with which inputs are transformed into desired outputs.

To achieve greater reliability and efficiency in existing systems and to assure adequate reliability and efficiency in systems under development, it is helpful to understand and interrelate the conditions and factors that influence the performance of the system. One way of doing this with existing systems is to change a system's configuration or inputs and then to observe the functioning of the system under these changed conditions. However, this procedure is time-consuming, costly, and disruptive to systems in operation, and is impossible in systems under development, systems designed for one-time use, and systems designed for emergency conditions. A more useful procedure is to analyze systems as far as possible through conceptual representations.

During the past two decades many techniques have been developed for the analysis of complex man-machine systems through conceptual representation. One can even say that a whole body of knowledge has been developed toward this end. This body of knowledge and its techniques are variously known as operations research, operations analysis, operational research, operational analysis, management science, systems engineering, value engineering, and systems analysis. Each of these terms has some unique

meaning, but the differences between them are of no consequence for the problem to be discussed here. However, the child needs a name; therefore, we shall call this whole body of knowledge and techniques "systems analysis," and its practitioners, "systems analysts."

In spite of all the refinements that have already been developed in the conceptual representation of complex man-machine systems, especially through the use of computer techniques, and despite the refinements that can be expected to be developed in the years to come, it is still true that any conceptual representation, since it is not the physical system itself, can represent only some of the system's attributes and interrelationships. But, to be meaningful, such a representation must structure all factors affecting the purpose for which the system is analyzed, and to be useful, the representation must be easily manipulated.

The preponderance of effort in systems analysis is directed toward making the representations or models easier to manipulate, and thus to permit the representation of larger numbers of attributes and interrelationships. Typical examples of this development are stochastic models, which became practical only with the introduction of high speed digital computers; and dynamic programming, which permits sequential approaches when the interrelationships are only partially known. Far less formal work has been directed toward developing techniques that would assure more meaningful representation. One reason for this is the feeling that the development of techniques that permit the manipulation of more complex models will, ipso facto, make the representation more meaningful.

Representatives of this approach are Richard Bellman and Paul Brock, who have developed numerous models and computer techniques for the solution of systems analysis problems. In a recent paper they discussed the concept of a problem and problem-solving.[*] Here they note that the steps involved in reducing a "natural problem" to a "symbolic problem" (i.e., a problem solvable by mathematical techniques) and in interpreting the solution in terms of the natural problem are non-trivial and require experience and skill. With this we surely can all agree. However, the authors then go on to state a criterion, their principal of balance,[**]

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* Richard Bellman and Paul Brock: "On the Concept of a Problem and Problem-Solving," American Mathematical Monthly, Vol. 67, No. 2, February 1960, pp. 119-134.
** Bellman and Brock state their principle of balance as follows: "A physically meaningful solution of a mathematical problem arising from a mathematical model of a physical process should never possess a greater degree of complexity than the mathematical model itself."

which can be used "as a rule-of-thumb guide" in the construction of
mathematical models which lead to a physically meaningful solution.
Their discussion, however, gives no approach or method except "experi-
ence and skill" by which one can proceed from a physical problem to a
physically meaningful solution, and even more important, they present
no alternative if their principle of balance cannot be met. This they
justify by noting that their approach is applicable only to those prob-
lems that can be formulated mathematically. The principle is thus a
vague guide for evaluating the efficacy of a model once it has been es-
tablished, rather than an approach or an aid toward the structuring of
complex systems.

This general concentration on the model rather than on the natural
problem appears to be a self-defeating approach to systems analysis. The
need for the analysis of complex systems arises whenever we try to under-
stand or predict their functioning. Usually some aspects of the system
can be mathematically analyzed; however, only rarely are all the aspects
of the system that affect the problem for which the system is being ana-
lyzed so well known or precisely formulated as to yield to rigorous mathe-
matical analysis. To exclude these less well-und stood aspects obviously
makes the analysis incomplete. And to justify such exclusion by stating
if "the problem is not well-defined I cannot solve it" begs the need of
those who are to be assisted by the analysis. Part of the function of
systems analysis is to take ill-defined and ill-structured problems and
attempt to structure them as well as possible. A systems analyst cannot
give up just because he is unable to achieve Bellman and Brock's "principle
of balance." He also cannot wait for further developments in the tech-
niques of mathematical modeling, as much as these future developments are
desired. The systems analyst must do the best and most meaningful job
now. To do this job he will have to use his "experience and skill."

But is this all we can say to him? Is the process of abstracting
from a physical system a conceptual representation so mysterious that it
contains no principles, approaches, methods, and techniques? Is systems
analysis a strict bifurcation between art and rigorous mathematical model-
ing? Or, does systems analysis consist of a blend of judgment and ra-
tional thought? I believe the latter to be the case. Formal work in
the methodology of systems analysis should therefore be directed not
only at developing techniques which permit the formulation and manipula-
tion of ever more complex mathematical models, but also at developing
techniques which give assurance that the abstractions of systems analysis
are meaningful to the solution of the actual problems encountered in com-
plex systems.

3

Here we are concerned with the development and justification of an approach that can lead to meaningful solutions of the multi-variable problems as they occur during the analysis of complex man-machine systems. To develop the requirements for such an approach, Chapter II compares science and systems analysis to discover the similarities and differences between the two endeavors. In that Chapter, I show that while both rely on empirical fact and rational thought, science is concerned with the developing of theories which describe the "world," while systems analysis tries to formulate adequate solutions to specific, predesignated problems. Methodologically, this difference implies that science is concerned primarily with the generalizability and verifiability of its theories, while systems analysis is concerned primarily with adequate solutions to problems as given. Systems analysis thus needs its own approaches as distinct from the methods of science. The matrix-network approach to the analysis of complex systems, which I describe in Chapter III, attempts to fill this need.

Since the primary objective of this approach is adequacy of the solution to the problem as given, the approach shows how relatively complete system structures can be developed and how the results of mathematical models, the most rigorous and thus preferred type of systems analysis, can be combined with the results of less formal analyses to form one systematic and integrated analysis of the over-all problem at hand. Since this approach is not completely formal, it must at various times rely on a ploy, the ploy of a knowledgeable analyst--that is, an analyst who acts not only as a decision-maker (effector), but also, through his judgment, as supplier of the decision criterion. One example of this occurs in the preliminary selection of the factors to be considered in the analysis, and in Chapter IV, Leonard Wainstein, a political scientist, describes how the analyst can use the highly informal but extremely insightful case study method as an aid in this phase of structuring complex man-machine systems.

That the occasional reliance in the matrix-network approach on the analyst's judgment as the decision criterion is not a unique weakness of this approach is demonstrated in Chapter V by C. J. Erickson, an anthropologist, and myself. Here we show, using phonology as our example, that the same judgment function can occur also in formal scientific analyses, where the emphasis is on rigor. However, while this judgmental element, the knowledgeable analyst, may not represent a unique weakness, the knowledgeable analyst is the logical and methodological mystery in this approach and as such requires a more satisfactory explanation than he receives in Chapter III. In an attempt to supply such an explanation, I asked two psychologists to discuss this problem from their respective points of view. First, in Chapter VI, Maurice Rappaport attempts to give a better understanding of the decision-making process

4

which the knowledgeable analyst employs by examining the psychological milieu in which the analyst operates. Here Rappaport focuses attention on the psychological processes associated with decision-making, especially those connected with finding a problem, problem-structuring, and purposive problem-solving. In Chapter VII, John B. Fink describes the judgmental function through a systematic behavioristic examination of the knowledgeable analyst. Fink uses a stimulus-response discrimination model to show that one can describe and define the operations which the knowledgeable analyst must perform, and that on the basis of this model, one can state procedures for systems analysis. In Chapter VIII, the final chapter, I attempt to draw conclusions about the nature of the knowledgeable analyst's judgment under consideration of the arguments advanced by my coauthors and myself in the previous chapters. On the basis of these conclusions, I develop a requirement which must be met by every successful approach to the structuring of complex systems.

II SCIENCE AND SYSTEMS ANALYSIS
by
K. H. Schaeffer

The methods of any human endeavor are determined by the objective for which the endeavor is undertaken. Thus the objectives of systems analysis will determine the methods and approaches required by systems analysis. If the objectives of systems analysis are identical in form to those of the sciences, systems analysis can effectively employ the scientific method. However, to the extent that this identity in form does not exist, systems analysis may not be able to use the scientific method effectively but may require its own unique methods.

In the following paragraphs I shall attempt to show that in fact the objectives and methods of science and systems analysis are not identical, and that the difference in their methods is especially apparent in the selection of facts to be considered and in the structuring of these facts.

This thesis is contrary to much common belief. The phrase "management science" tries to indicate by its name that we are dealing with a science, and Churchman, et al., in their Introduction to Operations Research, not only insist that operations research is a science but treat this as an unargumentative assertion.[*] Likewise, the Operations Research Society of America stated in its 1952 constitution (Article II): "The object of the Society shall be the advancement of the science of operations research . . ."[**] However, even within the professional organizations, doubts have arisen as to whether this discipline is truly a "science." Thus the new constitution of the Operations Research Society of America, which was presented to the membership in 1961, rephrases

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* C. West Churchman, Russell L. Ackoff, and E. Leonard Arnoff, Introduction to Operations Research, John Wiley & Sons, Inc., New York, 1957, p. 3.

** "Proposed Revision of the Constitution and By-Laws," Bulletin of the Operations Research Society of America, Vol. 9, Supplement 1 (Spring 1961), p. 62.

Article II to read: "The purposes of the Society shall be the advancement of operations research . . ." dropping all references to "the science of."[*]

There are differences between the scientific method and the method of systems analysis, but the two methods also have much in common. Both methods predict events, and both methods rest on empirical data and exact reasoning. With these large common elements it is not surprising that honest confusion has arisen. In addition, it should not be overlooked that "science" and the "scientific method" are "o.k. words," and since most analysts have been trained in one of the scientific disciplines it is usually considered to be more expedient to foster rather than to destroy the halo which science can give to systems analysis.

The Objectives of Science

The objectives of science can be examined from at least two distinct points of view. One can ask, "What are the objectives of science as an activity within the larger social context of other human endeavors--that is, what are the objectives of functions of science within society?" or one can ask, "What are the objectives of science within the context of science itself--that is, what are the objectives of the scientist in his role as a scientist when he is practicing science?" Especially since the last war, much has been written and many a discussion has been precipitated on the objectives of science from the first point of view, but for our present inquiry only the latter point of view is of consequence, even if far fewer words have been written about it, especially in popular and semipopular writings.

Among the modern writers who deal with the objectives of science from this second point of view, we find by no means a unanimity of thought but still a rather consistent theme. Some writers feel that science has two objectives, and this view is probably most eloquently stated by Einstein:

> The larger part of physical research is devoted to the development of the various branches of physics, in each of which the object is the theoretical understanding of more or less restricted fields of experience, and in each of which the laws

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[*] "Proposed Revision of the Constitution and By-Laws," Bulletin of the Operations Research Society of America, p. 62.

8

and concepts remain as closely as possible related to experience. It is this department of science, with its ever-growing specialization, which has revolutionized practical life in the last centuries, and given birth to the possibility that man may at last be freed from the burden of physical toil.

On the other hand, from the very beginning there has always been present the attempt to find a unifying theoretical basis for all these single sciences, consisting of a minimum of concepts and fundamental relationships, from which all the concepts and relationships of the single disciplines might be derived by logical process. This is what we mean by the search for a foundation of the whole of physics. The confident belief that this ultimate goal may be reached is the chief source of the passionate devotion which has always animated the researcher.[*]

If, along with Einstein, one divides the sciences into the specialized sciences and a unified base science, then it is beyond dispute that if systems analysis is a science it belongs to the former class. Its objectives as such would be, to paraphrase Einstein, the theoretical understanding of a more or less restricted field of experience through laws and concepts that remain as closely as possible related to experience. The important emphasis, however, even in these specialized sciences, is on the "theoretical understanding."

Popper emphasizes this view when he states that "the empirical sciences are systems of theories." Popper describes these theories informally as "nets cast to catch what we call 'the world': to rationalize, to explain, and to master it. We endeavor to make the mesh ever finer and finer." More formally Popper informs us that "scientific theories are universal statements."[**]

A different approach is taken by Kemeny.[***] In an attempt to answer the question "What is science?" Kemeny notes that since science embraces

[*] Albert Einstein, "The Fundaments of Theoretical Physics," Readings in the Philosophy of Science, edited by Herbert Feigl and May Brodbeck, Appleton-Century-Crofts, Inc., New York, 1953, pp. 253-54.

[**] Karl R. Popper, The Logic of Scientific Discovery, Hutchinson & Co., Ltd., London, 1959, p. 59.

[***] John G. Kemeny, A Philosopher Looks at Science, D. Van Nostrand Co., Inc., Princeton, N.J., 1959, p. 85.

the whole field of factual knowledge, it cannot be defined by its subject matter (since it has none of its own) but only by its method. Thus, according to Kemeny, any activity which employs the scientific method is a science. This method Kemeny characterizes in turn as follows: "First of all the scientist is an observer. Next he tries to describe in complete generality what he saw, and what he expects to see in the future. Next he makes predictions on the basis of his theories, which he checks against facts again."

Even so, Kemeny feels that science can only be defined indirectly, while both Einstein and Popper offered direct definition; all three approaches insist that science does not deal with the specific event but with general descriptions, universals, the theoretical.

The writers cited so far all tend toward a realistic approach to science; thus one may feel that this consistency in their themes is traceable to this common approach. However, one can find the same common theme among writers who tend toward a nominalistic approach to science. Since terms like "general" and "universal" are anathemas to nominalism, the emphasis of these writers is rather on the fact that science does not consist of specific events.

For instance, Chwistek tells us that physical theories are pure abstractions "which one cannot even regard as images of reality and that their rule reduces to this, that they make possible the systematic classification of phenomena as well as the investigations directed toward the discovery of unknown phenomena."[*] This statement indicates that, while the "rule" makes the body of science applicable to specific situations, the body of science is made up of pure abstractions (i.e., descriptions in complete generality, in Kemeny's terminology), which are not even images of the reality (the specific).

Cohen and Nagel write that "the ideal of science is to achieve a systematic interconnection of facts. Isolated propositions do not constitute a science. Such propositions serve merely as an opportunity to find logical connection between them and other propositions."[**] Kemeny defined science by its method; Cohen and Nagel, however, note that the

* Leon Chwistek, The Limits of Science, Routledge & Kegan Paul Ltd., London, 1949, p. 3.

** Morris R. Cohen and Ernest Nagel, An Introduction to Logic and Scientific Method, Harcourt, Brace and Co., New York, 1934, p. 394.

scientific method has applications outside of science, and that only "the most striking applications of scientific method are to be found in the various natural and social sciences."*

We can thus conclude that, regardless of whether one's approach to science is realistic or nominalistic, or whether one believes there are many separate sciences or one theoretical basis, or whether one believes science is the only exemplification of the scientific method rather than one of a number of exemplifications, there is always the theme present that science does not have its objective in the specific, the particular fact or event, but rather in something more systematic, abstract, general, or universal.

The Objectives of Systems Analysis

The objectives of systems analysis are the analyses of particular and specific situations in order to ascertain how adequately these situations or systems meet certain problems.**

These objectives differ from those of science in two important aspects. First, systems analysis deals with the specific, the particular, the unique. Its interest in the general description, the universal, and

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* Op. cit., p. 392.

** Some will consider this statement too restrictive since it does not include the selection of the "optimum" situation. I omitted this decision-making process between alternatives for two reasons. First, this process is syncretic rather than analytic in nature. Thus the logic and methods required for it are quite different from those required in analyzing how a particular situation meets any given set of problems. Second, while the meaningfulness of this synthesis depends on the meaningfulness of the analysis which preceded it, the contrary is not the case. Thus one can develop a method for structuring and analyzing complex systems without considering the problem of selecting the "best" system from a number of alternatives. However, it is not possible to develop a meaningful method for selecting between alternative systems if one has not previously or simultaneously developed a meaningful method for structuring and analyzing one system. This restrictive statement, therefore, focuses attention on the truly analytical part of systems analysis.

the abstract is confined to those occasions where these general truths
or hypotheses permit a deduction toward the specific. While systems
analysis uses the general as a means in the determination of the specific,
in the sciences the specific and isolated fact is used as an element or
means in deriving the general and abstract description or hypothesis.
Thus, what is for one type of discip.ine the end is for the other type
a means, and vice versa.

The second difference between the objectives of science and systems
analysis is in "the problem." Some writers on the philosophy of science
emphasize that any scientific inquiry must begin with a problem.[*] The
types of problems from which scientific inquiry arises appear to have
invariably as their solution a "theory" in Popper's meaning of the term.
Thus a problem in science is solved if it is understood in terms of a
general theory. By contrast, in systems analysis the solution to a
problem is the meeting or avoidance of the predesignated problem in
terms of some purpose. While understanding may be a significant step
in achieving the objectives of systems analysis, it is in this context
only a means to an end, and not an end or objective in itself.

The first questions asked about any problem in systems analysis
concern the criteria by which the problem is to be analyzed. These
criteria in turn are really expressions of the purpose for which the
problem is to be analyzed. For instance, if the problem is to analyze
an inventory control system, the purpose for conducting this inquiry
could be one of many. To mention just two, the purpose might be to de-
termine the relationship between service and expenditure in an effort
to maximize service while minimizing expenditure, or the purpose might
be to determine the most appropriate inventory control system within
the over-all company objectives. A possible answer to a study conducted
with the second purpose in mind might be to propose an "instant" manu-
facturing process which would eliminate an inventory altogether, thus
avoiding the problem rather than meeting it. There may be some general
theories which relate inventory control system service to expenditure or
to other facets of company operations; however, such general theories
are not sufficient for the solution of the specific problem with which
the systems analyst is confronted. What the systems analyst needs are
statements that relate the problem to all the conditions that confront
those facets of the system which are involved in the problem. Whatever

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* See Cohen and Nagel, op. cit. and Percy W. Bridgman, Reflections of
a Physicist, (New York: Philosophical Library, 1955).

12

is needed to enumerate these conditions fully--or at least adequately for an acceptable solution--must be included in the analysis. The systems analyst will try to relate the various isolated statements of conditions to one another and to show specific and possibly general relationships between them. However, the fact that he may not be able to arrive at general relationships does not permit him to limit his problem to those factors for which general relationships can be derived. By contrast, the scientist is interested solely in the establishment of general theories and therefore must limit the selection of the factors he will consider to those which have bearing on the affirmation or contradiction of one or more general theories.

While there are these differences in the objectives of science and systems analysis, the two objectives also have some common elements. The most significant of these common elements is the fact that both types of disciplines have no final solutions. No problem in either science or systems analysis is ever completely solved. Due to the interdependence of the various sciences, no scientific theory will ever be the final truth until all scientific theories are known. Likewise, due to the impossibility of limiting a problem in systems analysis except arbitrarily, no problem of the real world--be it specific or general--can ever be fully understood until it is related to all other facets of the world. Thus all solutions in science as well as in systems analysis are temporary solutions--solutions which for the time being are adequate, but which never are final.

Differences in the Methods of Science and Systems Analysis

The objectives which one attempts to achieve, whether in science, systems analysis, or any other human endeavor, determine in part the means or methods used in the realization of the objectives. Since the objectives of science and systems analysis differ from one another--in spite of large common elements--it is to be expected that the methods of achieving the respective objectives will show differences as well as similarities.

The two methods are similar because both involve prediction and both are based on empirical fact and rational thought. The two methods are most strikingly different in their approaches to the definition of "fact," the selection of relevant facts, prediction, and verification.

13

Definition of "Fact"

Since the sciences include such diverse fields as astrophysics, nuclear physics, biology, and psychology, questions such as "What exists?" "What is real?" "What is a fact?" are not easily answered in statements which deal universally with all sciences. The first approach of the sciences to the problem of existence is the assumption of a common-sense realism. Such a realism assumes that the various objects of the external world exist independently of the knower. Furthermore it is assumed that this existence can be verified objectively through direct observation. These two assumptions, however, cannot be generalized for all scientific inquiries. For instance, while I know that I am conscious, or while I know I feel joy and pain, the scientist cannot make statements of this type about other human beings and still insist on objective verification. The scientist can objectively verify the behavior of human beings but not their sensations, unless he first translates these sensations into objectively observable behavior patterns. While a scientist can verify the reality (that is, the existence) of dogs and cats through direct observation, such direct verification is not possible for subatomic particles. While we assume, in our common-sense approach to the external world, that time and space are independent, linear, and infinite, such assumptions are no longer tenable for the scientist who deals with galaxies.

Because of these difficulties, the writers on the philosophy and the logic of science have generally abandoned common-sense realism in explaining scientific facts, and have had recourse to theories which in some way combine the problems of existence and verification. Extreme examples of theories of this type are operationalism and instrumentalism. Also, because of these difficulties, there is a tendency to evaluate theories in the philosophy of science by their ability to deal with the problems of nuclear physics and astrophysics.

The systems analyst, in contrast, restricts himself arbitrarily to man-machine systems which consist solely of components whose existence is directly observable. By introducing this restriction he is, on one hand, gaining the opportunity to deal within each system with a common-sense approach to the problem of "What exists?" "What is a fact?" On the other hand, he is forced to eliminate from his inquiry extreme macro- and microcosmic systems such as the total universe and the atom. This limitation of systems analysis does not imply that these are not really systems; it just implies that these are not systems with which systems analysis--as delineated here==can be concerned.

14

The insistence on a common-sense view of reality permits the systems analyst, as part of his analysis, to make such statements as "There is a computer" without necessarily having to relate this computer to anything or to measure the existence of the computer in any manner or form whatsoever. For the systems analyst the existence of the computer can be treated as an isolated fact. This fact may be related to some other facts, but to be a fact it need not be related to anything. This rather naive approach can of course be questioned from many different points of view. However, since the systems analyst can restrict his inquiry to those systems in which this naive approach need not be questioned, he can utilize this common-sense acceptance in an approach toward structuring complex man-machine systems (which will be outlined in Chapter III). Another advantage of the common-sense approach to reality lies in the willingness to accept the existence of certain facts without insisting that they are objectively verifiable. Thus the systems analyst can accept the subjective approaches of the human component within the system, and is not forced to the scientific circumlocution of subjective statements through objectively verifiable measurements.

So it is that while the scientist can consider as a fact only that which is verifiable or measurable, the systems analyst can consider as a fact that which he considers to exist apart from his observations and measurements. The systems analyst's definition is simpler (i.e., more simple-minded) than the scientist's, but is tenable only in a limited range of human experience. However, since the systems of systems analysis fall within this range of experience, such an approach is adequate.

Selection of Relevant Facts

It is a well-known dictum that to know everything about something implies that one know everything about everything. All of our experiences and all of nature are so interrelated that nothing can be completely isolated from anything else in every respect except through some arbitrary decision. Thus neither science nor systems analysis can include "all" that relates itself to the world or to a problem. Both types of endeavor must deal with sets of relevant facts selected from an infinite sea of facts.

To answer the question for science, we can turn to Poincaré, who devotes his entire book on Science and Method* to a discussion of "how

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* Henri Poincaré, Science and Method, translated by Francis Maitland, Dover Publications, Inc., New York.

15

the scientist is to set about making a selection of the innumerable facts
that are offered to his curiosity . . ." Summarizing his argument in the
final chapter, he states:

> There is a hierarchy of facts. Some are without any positive
> bearing, and teach us nothing but themselves. The scientist
> who ascertains them learns nothing but facts, and becomes no
> better able to foresee new facts. Such facts, it seems, occur
> but once, and are not destined to be repeated.
>
> There are, on the other hand, facts that give a large return,
> each of which teaches us a new law. And since he is obliged
> to make a selection, it is to these latter facts that the
> scientist must devote himself.[*]

Thus while the scientist can consider a fact relevant if the fact gives
a large return in terms of new laws and theories, the systems analyst,
having other objectives, must use different criteria for determining
whether or not a fact is relevant.

Like the scientist, the systems analyst has no interest in facts
because they are facts. Facts to him, as to the scientist, are only
means toward ends. Since the systems analyst's ends are to study spe-
cific systems for the purpose of solving specific problems, he needs to
be interested in all those facts--and only those facts--that bear on the
solution of the specific problem and system under consideration.

When the analyst's field of interest is narrowed to one or a set
of specific problems of a given system, some of the facts which are on
the bottom of Poincaré's hierarchy may suddenly gain prime importance.
For instance, one fact about the system may be that it must be opera-
tional on day D, or that the cash budget for operating the system may
not exceed y dollars. These two statements are specific facts, facts
with which no power to generalize can be associated, and facts for which
no generalization may be intended. Thus the fact that the system must
be operational on day D may just as well be associated with the state-
ment that the system must be operational on D+1 as with the statement
that the system must not be operational on D+1, or even with the state-
ment that it is neither highly desirable nor undesirable if the system
is operational on D+1. The fact that the system must be operational
on day D, while not a generalizable fact, is a fact that may well be a

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* Ibid., pp. 284-85.

major restraint on any proposed solution to the system problem under discussion. Thus the fact is important and relevant to the systems analyst while of no importance to the scientist. An analogous argument could be developed for a cash budget limit of y dollars.

Conversely, facts of great relevance to the scientist may be of minor importance to the systems analyst. A scientist who attempts to establish a general theory about some events in nature will attempt to recreate these events if possible in a strictly controlled environment, and then on the basis of the measurements obtained in this controlled environment--in common parlance, the scientific laboratory experiment-- will enunciate a general theory. The systems analyst lacks the controlled environment; he must deal with systems and problems as given, not as "defined" or "established." In his complex environment, the importance of the general theories describing scientific abstractions of the actual situation may be overshadowed by other--that is, special-- circumstances to such an extent that the general theories are useless.

For instance, the physicist tells us that the terminal velocity of free-falling bodies in vacuum can be expressed as a function of gravity (g) and the height of fall (s); thus:

$$v = \sqrt{2\ s\ g}$$

Let us now assume that the systems analyst is concerned with the impact speed of certain free-falling feathers and stones under wind conditions. In this case, v is primarily a function of the density of the falling bodies, wind direction, and wind velocity, rather than of gravity and height of fall. This specific case is of course in no way a denial of the theory of free-falling bodies, and theoretically the case can be described as a more complex case of free fall. However, let us recall that the essence of Galileo's discovery was the elimination of the density parameter from the general description of free-falling bodies. Galileo showed through controlled experimentation--which has since been substantiated over and over again--that for general scientific considerations, distance (or gravity and time) is the prime factor affecting the terminal velocity of falling bodies. Density was shown to be a factor affecting only certain falls, not a factor affecting all falling bodies. By Poincaré's definition, the facts of distance and gravity are more general facts than the fact of density, and thus are higher on the scientist's hierarchy. For the systems analyst in the specific case cited, however, the facts of density, wind velocity, and wind direction have greater relevance than the facts of gravity and distance.

17

Unfortunately, many an analyst, driven by the desire for "scientism" rather than the desire for rationalized problem solving, tends to begin his analysis with the abstractions science offers rather than with the problem as given. Where this occurs, he offers neither good science nor good systems analysis, but irrelevant facts and theories which at most are oversimplified analyses and solutions. The producer--and thus defender-- of these irrelevances tends to justify himself by claiming that if he had had more time and money he could have gone into greater detail and pro- duced a more meaningful analysis. However, an examination of his methods and techniques usually does not bear out this assertion. What he did was to force the specific problem to fit his generalized analysis rather than fit the analysis to the problem. If he had had an infinitely greater amount of time and money, he might finally have been able to refine his "scientific approach" structure to such a degree that it could handle a whole class of specific problems, including the one problem assigned to him. With only a finite amount of time and money available--and alas, usually far too finite--it would be mere chance if this scientific course of fact selection and generalization led our analyst to an adequate solu- tion. Systems analysis must, as far as possible, structure the problem as given rather than treat it as an instance of a general class of prob- lems.

Prediction

Both science and systems analysis involve the prediction of future events. However, in spite of this similarity, prediction is also one of the areas in which the two disciplines differ from one another. The scientist predicts only those types of events which he can predict, whereas the systems analyst predicts those types of events he must pre- dict. If the scientist lacks the theory to predict certain types of future events, he refuses to predict them, excusing himself by stating that at this time we lack the theories necessary to predict events of this type.[*] The systems analyst, however, must predict. It is in many respects his role to be the fool who rushes in where angels fear to tread. Where he cannot measure, he may even guess. However, to keep his argu- ments as rational as possible, the systems analyst will restrict himself

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* The philosophical problems associated with prediction in the sciences are omitted here; since each one of these problems is shared by sys- tems analysis. These problems present therefore no distinction be- tween the methods of science and systems analysis.

18

to the narrowest basis possible. Thus the analyst will try to predict not a whole host of situations, but the least number of situations to which he can meaningfully restrict his analysis.

Statistics is the tool which, under the requirements of science, has been developed for describing rigorously and rationally the arguments that lead toward the rational prediction of future events. This tool has been applied to scientific problems as well as to those of systems analysis--in many respects maybe even more frequently to the problems of the latter than the former. However, the use and especially the interpretation of statistics raises some questions when applied to certain types of problems which are not infrequent in systems analysis. The systems analyst is frequently called upon to analyze a system in relation to a one-time occurrence. "How will this system perform if its vehicle is on the first flight to the moon?" "How will this defense system react to the first enemy surprise attack?" If the analyst answers these questions by formulating a probability model based on valid and consistent assumptions, he says in effect that there is a class of possible systems performances (or reactions) and the member of this class most likely to occur is "a." Now let us assume that the system performs (or reacts) but the results are "b," which according to the analyst was a possible--but not the most likely--event. Was the analyst in his analy-sis right or wrong? From the point of view of statistics, the analyst can only be shown to be wrong empirically if a large set of "first flights" or "first enemy surprise attacks" occurs and the distribution of the events is significantly different from those forecast by the analyst. Since "firsts" are one-time events, this type of empirical verification is impossible. In accordance with this type of reasoning, the analyst can always claim to be right as long as the event which occurred was one of the events which he considered possible, regardless of the proba-bility which he assigned to its occurrence.

By contrast, those who commissioned the analyst will consider the analyst's answer to have been wrong. These people will insist that they asked the analyst what will occur, not what is most likely to occur, and since his answer did not list the event which occurred, but rather another event, the analyst in the eyes of the world is judged to be wrong. The analyst naturally will complain about such a "misinterpretation" of his results. But how can he, if he dared to take credit when his prediction of the most likely event was truly the event that occurred?

The solution to this difficulty can hardly be that in relation to the event which occurs the analyst is neither right nor wrong. If this were so, then the analyst's analysis would be meaningless. To have

meaning, the analysis must stand in some relationship of validity to the actual event. What this relationship is I do not know. All that can be said here is that the meaningful prediction of one-time events is a problem for which no satisfactory solution has so far been presented.

Verification

Some logical positivists have gone so far as to insist that only empirically verifiable statements are meaningful.[*] Regardless of whether one cares to share this extreme point of view, it has long been recognized that empirical verifiability is an essential attribute of the statements of the empirical sciences. Thus the scientist in formulating his theories must state them in terms that are at least potentially empirically verifiable. The effect this requirement has on the scientist's definition of fact has been briefly indicated on the foregoing pages.

For the systems analyst, verifiability does not occupy a central position. To be sure, the systems analyst would like to state his conclusion in quantitative statements which are empirically verifiable, but unlike the scientist, he will not insist that he may formulate no other types of conclusions.

Except for the difference in emphasis, science and systems analysis do not differ in their views on what is verifiable, nor on how a statement can be verified. Thus one is tempted to regard this difference in emphasis as a minor difference. However, the fact that verification is at the very heart of the empirical sciences while it is on the fringes of systems analysis makes this in some respects the most crucial of the four differences. The differences between the two disciplines on the definition of fact and on the admissibility of certain not directly verifiable facts would not be possible if it were not for the difference in emphasis on verification. Thus this difference in accent, which at first glance appears to be so minor, may actually be the most fundamental difference between the two endeavors.

Conclusion

The objective of science is the development of theories which describe the "world," while the objective of systems analysis is the formulation of

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* See, for example, A. J. Ayer, Language, Truth and Logic, Dover Publications, Inc., New York, 1950.

adequate solutions to specific, predesignated problems. This difference in objective leads to a difference in method. While the methods of the two endeavors are similar in their reliance on empirical fact and national thought, they differ in their definition of fact, fact selection, probability, and verification. Two of these differences (definition of fact, and verification) demonstrate the systems analyst's greater concern to arrive at some answer--a partial answer being better than none--and his lesser concern with a rigorous methodology. The other differences (fact selection, and prediction) bring out the systems analyst's concern with specific truth applicable to one-time events, in contrast to the scientist's concern for general truth applicable to an infinite set of events.

It is in the areas of fact selection and prediction, then, that systems analysis needs different concepts and different approaches from those of science. In an attempt to evolve a general method for systems analysis, the following chapter presents an approach to the systematic selection, structuring, and analysis of facts in systems analysis.

III THE MATRIX-NETWORK APPROACH TO THE ANALYSIS
OF COMPLEX SYSTEMS
by
K. H. Schaeffer

The approach described here has much in common with the Systems
Analysis and Integration Model (SAIM) developed by Albert Shapero,* and
in the discussion which follows I am indebted to Shapero and to SAIM--a
debt which I gratefully acknowledge. The first two steps of the approach
taken here are nearly identical with SAIM's initial operations, but it
is still well to distinguish between the two approaches since their ob-
jectives are different. SAIM was designed to be employed as a self-
contained tool in the analysis, synthesis, evaluation, planning, and
management control of weapon systems, and it has been shown to be highly
successful in these applications. By contrast, we are concerned here
with an approach toward structuring and analyzing systems, so that pre-
designated problems can be solved wherever possible through the formu-
lation of meaningful mathematical models--the standard method for rig-
orous analysis--and where this is not possible, through the use of a
systematic framework for connecting formal analyses with informal judg-
ments.

    This approach is based on the assumption that there are systems,
and that these systems can be conceived to consist solely of elements
and direct relations between element pairs. This implies that all com-
plex relations within the system or affecting the system can be described
in terms of these elements and direct relations. The purpose of the ap-
proach, then, is the systematic determination and analysis of the elements
and their direct relations which constitute any given system if it is
analyzed in respect to some predesignated problem or problems.

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*   For a description of SAIM with special reference to its use, see
    Albert Shapero and Charles Bates, Jr., A Method for Performing Human
    Engineering Analysis of Weapon Systems, WADC Technical Report 59-784,
    Aerospace Medical Laboratory, Wright Air Development Center, Wright-
    Patterson Air Force Base, Ohio, September 1959. A briefer discussion
    is contained in K. H. Schaeffer and Albert Shapero, The Structuring
    and Analysis of Complex System Problems, Air Force Technical Note
    AFOSR 810, Stanford Research Institute, Menlo Park, California, under
    Contract AF 49(638)-1020 to the Air Force Office of Scientific Research,
    May 1961.

The employment of this approach involves the following steps:

Step One   Preliminary selection and classification of the system
elements affecting the predesignated problems

Step Two   Preliminary determination of the existence of direct
relations between element pairs

Step Three   Restatement of the system elements and direct relations
to achieve greater consistency between elements and
simpler compound relations

Step Four   Mathematical modeling of those elements and relations
that lend themselves to an analysis of this type

Step Five   Evaluation of the completeness or adequacy with which
the mathematical models represent the elements and
relations which are included within or subsumed by
these models

Step Six   Description of the direct relations that are not or
are only partially represented by the mathematical
models

Step Seven   Judgment integration of the mathematical models and
the additional descriptions

The Assumptions of the Approach

By its name, systems analysis affirms the existence of systems.
However, to arbitrarily delineate the meaning of systems is of little
avail, since this meaning, like those of all broad concepts, is shrouded
in large gray areas. For the present it is sufficient to characterize
a system as a potential or actual physical complex which is considered
in relation to some process. On the basis of this characterization we
can speak of weapon systems, transportation systems, educational systems,
production systems, library systems, filing systems. However, this
characterization is not so broad that it includes any assemblage of
physical objects as a system. Thus, for instance, the typewriter by
itself is not a system unless a process can be associated with it. If,
however, this process is "typing," the physical objects making up the
"typing" system include--besides the typewriter--at least the typist,
her chair, and the platform supporting the typewriter.

24

Conversely, when we speak of ideological systems, political systems, or social systems, we tend to refer to collections of processes, functions, and concepts which are disassociated from specific physical complexes. Such systems would not fall among those which necessarily can be studied by the present approach, since this approach is devised to handle systems that contain actual or potential physical entities. While the approach can be applied to all systems containing physical entities, it is primarily directed at complex man-machine systems.

Although a system may form a whole, it cannot be understood as a whole but must be partitioned into parts which, if connected properly with one another, will convey the concept of the system as a whole. The usefulness of this partitioning depends on the degree to which the sum of the parts equals the whole. Here we will make the working assumption that total identity exists between the sum of the parts and the whole.

Many philosophical objections can be raised against the validity of this assumption, but the assumption is a workable one, since every one of the steps requires to a greater or lesser degree the judgments of a knowledgeable analyst. Through these judgments by adding, subtracting, or combining parts, the analyst can, wherever necessary, adjust the balance between the sum of the parts and the system as a whole so that a meaningful identity exists. Without this judgmental process the identity between the sum and the whole can be maintained only by definition--in which case, the common-sense denotation of the whole may be at variance with the defined denotation of the whole. To the degree that this variance arises, the concept which is being analyzed and the common-sense concept of the system will differ from one another and the analysis will lose realistic meaning, and thereby, practical usefulness.

Where the end products of systems analyses have been decried because of gross oversimplification, it has usually been the case that the sum of the parts was not in correspondence with the common-sense concept of the system as a whole. While at times judgment can correct this imbalance, judgment (since it is nonrepeatable and thus unstable) should be resorted to only where rational thought is inadequate. Thus the present approach--as with any other approach to systems analysis--will be at its best if it dispenses with ad hoc judgments and still is meaningful. In short, we must take an atomistic concept of systems in order to be able to analyze them rationally, but on the other hand we must not be bound by this approach to such a degree that the analysis loses practical meaningfulness. Thus the approach of analysis through parts is used here as a working assumption, not as an assumption about the nature of systems.

In the present approach, the <u>system elements</u> are the parts into which a system is divided for analysis, and the <u>direct relations</u> are the connections between the elements; together, these represent the total system.

The elements include the physical entities which make up the system, the processes and operations which connect these entities, and the forces and factors external to the system which affect or are affected by the existence and operation of the system. Thus for purposes of analysis the environment in which the system operates is considered to be part of the system.

The specific elements considered in the analysis will depend on the purpose or problem for which the system is studied. For instance, if a weapon system is studied to determine its operational effectiveness, the elements representing the physical entities will tend to be the operational subsystems of the system, and these may be divided into equipments and operators. However, if the weapon system is analyzed to ascertain its maintenance problems or spare part support requirements, the elements representing the physical entities will be the maintenance personnel, the maintenance modules, or the parts which are replaced rather than repaired in the maintenance process. The same variability in breakdown occurs if we consider the external forces affecting the system or the operating procedures of the system. Thus the question: "How many elements has system X?" is a meaningless one, since the number of elements considered is dependent not only on the system but also on the purpose for which the system is studied. One can say, however, that on the one hand each system should be broken down into as few elements as possible--by the same concepts of parsimony as apply to the sciences--while on the other hand the elements should be sufficient in number to make them and their direct relations meaningful representations of the total system.

What are these direct relations which connect the various elements with one another? If the elements are considered by themselves, they represent a collection of the parts of the system--a collection without any particular structure. This collection would have as much or as little to do with the system as a whole as a pile of building materials has to do with the finished building. As from a pile of building materials many different types of buildings can be constructed, so from a collection of elements many different systems can be formed; and as the various building materials must be placed in a certain relationship to one another to form a building, so the system elements must be connected by a given set of relationships to form the system. The set of relationships through which this end is assumed to be achievable is the set of direct relations.

Any particular direct relation exists only between two system elements. Operationally, element A is said to be in direct relation to element B if a change in A affects a change in B without <u>necessarily</u> affecting any change in any other element of the system, unless such a change, in turn, is affected by a change in B. We say that a change in element A <u>affects</u> a change in element B, rather than that a change in element A <u>effects</u> a change in element B, in order to avoid the implication that this relationship is necessarily causative. Throughout, we shall use the terms "to affect" and "the affect" if we wish to imply a broader relationship than a causative one. To use the terms "to effect" and "the effect" implies that we are solely concerned with causative relations, which in this discussion is not the case.

It should also be noted that the definition of direct relation does not imply that if A is in direct relation to B, then B is also in direct relation to A. If the second assertion is correct, two direct relations exist. Furthermore, by definition, A is never considered to be in direct relation to itself.

In making the direct relation the fundamental and sole class of connectors within the system, we say in effect that every relation within a system involving three or more elements (that is, involving a compound relation) can be broken down meaningfully into a set of direct relations. Thus there are three possible interpretations of the statement that a change in A affects C only if a change in B also occurs:

1. The change in A affects a change in B which affects a change in C.

$$A \Longrightarrow B \longrightarrow C$$

In this case A is in direct relation to B and B is in direct relation to C, while A is in compound relation to C.

2. The change in A which will affect C requires as a precondition a change in B which affects A.

$$B \longrightarrow A \longrightarrow C$$
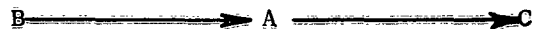
In this case B is in direct relation to A and A is in direct relation to C, while B is in compound relation to C.

3. While A affects C and B affects C, the change in A does not affect B nor does the change in B affect A.

A —————————————→ C
B —————————————→
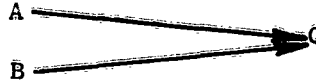
In this case, A to C and B to C form direct relations, and no compound relation exists.

Objections to this last point of view tend to be based on the observation that the change in A may be too minute to cause any change in C, and that the same can be said about the change in B, and that only if the two changes occur together can their combined occurrence produce a change in C; thus the two relations cannot be considered in isolation from one another. This objection can be met by noting that we are here concerned with the existence of relations and not with quantitative descriptions of these relations. The two relations taken separately may be too weak to cause a change in C, but they can exist independently of each other if they are regarded solely as affecting (contributing to) a change. While no single drop of rain causes a cloudburst, each drop affects the cloudburst, if ever so minutely. Since these minute direct relations must be considered (maybe only to be discarded later in the analysis), we will note in Step Two that it is operationally more feasible to determine that no direct relation exists between a given pair of system elements, than to determine that a direct relation exists.

The Steps of the Approach

Each step in the approach contains some formal and systematic manipulations, but each step to a greater or lesser degree also involves some judgment on the part of the systems analyst. The approach is therefore not a rigorous method, but a guide to systems analysis, and only an analyst knowledgeable of the system will be able to use these steps. Since no two human minds can be expected to agree in all their judgments, no two analysts analyzing the same system for the same problem can necessarily be expected, by following these steps, to arrive at the same conclusion. In the present chapter, little attention is paid to these judgmental differences--the emphasis is rather on the systematic and formal aspects of the various steps. Chapters IV-VIII will discuss the contributions of the knowledgeable analyst to the structuring and analysis of complex systems.

The first step is to identify,[*] select, and classify the system ele-
ments. While in theory the list of these elements can be of indefinite--
if not infinite--length, in practice the number of elements listed will
vary in inverse relation to the precision with which the problem, or the
purpose of the analysis, has been defined.

Since the meaningfulness of any systems analysis is to a great ex-
tent dependent on the care with which the facts included in the analysis
are selected, and since each systems analysis must be responsive to the
particular problems which are its special concern, great care needs to
be exercised to assure that a sufficiently complete list of elements is
obtained. To assure this adequacy the elements are not listed in random
order, but an element classification scheme is used to aid the systems
analyst in the selection of elements by reminding him of the types of
elements that need to be included.

How can the system elements be classified? Since there is an in-
definite number of elements, an indefinite number of classifications is
possible, and any one of these would be adequate if its sole purpo᠎ were
to remind the analyst of the areas in which he might find a likely systems
element for his selection. However, if we want the systems analyst to
think about the system in some systematic fashion while making his ele-
ment selection, the classification scheme should also guide the analyst's
thinking through the various mazes of the system. We know from the his-
tory of science that the most successful classification schemes have been
those which in their organization of facts foreshadowed the discovery of
some general laws, as, for instance, the Linnean classification system
in biology, and the periodic table in chemistry. By analogy, to con-
struct a "successful" classification scheme for the systems elements
would require a general systems theory applicable to all particular
systems.

In the absence of a general systems theory, no classification can
be constructed which is knowingly based on such a theory; thus no classi-
fication system can be constructed of which we can say a priori that it
will "guide the analyst's thinking through the various mazes of any sys-
tem." If a classification system were constructed that foreshadowed new

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* How case studies can assist in the identification of system elements
  is described in Chapter IV.

theories, we could not recognize this fact until after the theories had been derived either on the basis of the classification system itself or parallel to it. All that can be done at present is to devise a scheme which at least partially incorporates some of the concepts that appear to be common to all systems of interest to systems analysis and which incorporates concepts of significance to the subsequent steps.

All elements appear to be classifiable into three mutually exclusive classes: determiners, components, and processes. The determiners are those elements which affect the system from outside the system proper; they include the inputs that the system must accept, the outputs of the system, the objectives of the system in terms of the systems analysis, and the other constraints external to the system that operate upon the system, as for instance the forces of the natural or social environment into which the system is placed. The components are those elements which make up the actual physical entities of the system. These are the machines and equipment, the humans who direct, operate, or perform maintenance within the system, and the facilities that are internal and integral to the system. The third group of elements is comprised of the processes which are performed within the system. These processes include the physical entities processed through or changed by the system, the time sequences and operating procedures in which operations and actions occur within the system, the communications within the system, and quite generally all the processes which by themselves represent sufficiently meaningful entities to be considered units--or elements. There is always a gray area between those processes which form meaningful entities and thus are represented as elements, and those processes which are so vaguely defined that they are treated as relations rather than as separate elements.

The three categories--determiners, components, and processes--appear to have over-all validity since all systems of the type considered by systems analysis appear to have elements which fall into each of these classes, and all their elements appear to be classifiable--without force-- into these categories. It may be that there are subcategories within these categories that have equal universal validity, but, to date, no convincing arguments or theories of general applicability have been discovered to support such classifications. For special groups of systems which are studied for specific purposes, far more detailed element classification schemes can be developed.*

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* Shapero, in his Systems Analysis and Integration Model (SAIM), first used this threefold element classification. He also developed a further classification breakdown for weapon systems, which he analyzed with respect to human factor problems. (See Shapero and Bates, p. 7.)

The difficulty in establishing generally applicable subcategories can best be seen in the component category. The size, complexity, and grouping of the physical entities which will be called elements will, to a large extent, be functions of the purpose of the systems analysis. For instance, within a weapon system one can divide the component elements by operational subsystems or by types of equipment (as found in a parts catalogs) and job positions. Either set of subcategories will add up to the same component category, but, depending on the purpose of the analysis, one set may be favored over the other. Thus, while the subcategories selected are to a great extent determined by the objectives of the analysis, the three main categories appear to be independent of the objectives of the analysis and therefore appear to have universal applicability.

Thus this report can contain no further classification breakdowns. The systems analyst is still advised, however, to try to construct such further breakdowns for his particular system and problems before beginning with the preliminary selection of the specific elements affecting his analysis.

Step Two: Preliminary Determination of the Direct Relations

After a preliminary listing of the system elements has been made, each pair of elements is examined to determine whether a direct relation, as defined above, exists between the first member of the pair and the second member, and between the second member and the first.

Operationally this step is best performed if the elements are listed, as in SAIM, in an n by n matrix. If within this matrix the element represented by the ith row directly affects the element represented by the jth column, the corresponding cell is marked with a 1 (one); if the element in the row does not directly affect the element in the column, the cell is marked with a 0 (zero). The cells in the diagonal which represent the ith row and ith column are left blank, since an element is never considered to be in direct relation to itself. To illustrate this matrix, let us assume that we have an unusually small and simple system, consisting of five elements. For this system the matrix might look like this:

	A	B	C	D	E
A		1	1	0	1
B	0		0	0	1
C	0	0		1	0
D	1	0	0		1
E	0	1	0	0	

In this example, for instance, element A directly affects element B
(A⟶B); therefore, the cell defined by row A and column B is marked
with a 1. Since element B does not directly affect element A, the cell
defined by row B and column A is marked with a 0.

In performing this operation, we are interested solely in establish-
ing whether a direct relation exists between one specific element and
another specific element; we are not interested in the qualitative or
quantitative description of this relationship. Thus, in performing this
operation we are not concerned by the fact that we do not know the quan-
tities of the relationship or the dimension in which the relationship
can be measured. As important as these descriptions are, it is of even
greater importance that a relationship is not omitted from the analysis
because "we do not know how to handle it." In systems analysis the
awareness of the existence of such a difficult relationship is always
better than ignoring it in order to be able to solve the problem quan-
titatively. The approach thus offers an opportunity to list, in the
process of the systems analysis, every direct relationship regardless
of whether it is easy, difficult, or impossible to describe it precisely.

To think of the existence of something without at the same time at-
tributing to it a description entails well-known philosophical problems.
One may well ask what is meant by saying that a relationship exists with-
out at the same time implying a description of it. For this reason it

is a more meaningful operation to reverse the question and ask, "Is it a fact that I can postulate no way in which a change in element A can directly affect element B?" If the answer to this question is "yes," the cell is marked with a 0; and if the answer is "no," a 1 is placed in the cell.

The analyst, in thinking his way through Step Two, will usually find that he has omitted certain elements in Step One, or that certain entities of the system are better expressed through different sets of elements. Therefore, Step Two usually includes revision of the elements as listed in Step One. Naturally, this revision is not a logical but only a practical aspect of Step Two.

Step Three: Restatement of System Elements and Direct Relations

Since the elements were originally selected on the basis that they affect the purpose for which the system is being analyzed, and since this purpose is not necessarily well defined, it is quite possible that the elements as listed are not independent of one another, but overlap each other. Since such overlap in meaning can only lead to confusion in the process of the analysis, the elements are at this time re-examined and rephrased if necessary, so that the list of elements represents a list of terms in which each term has a specific and independent meaning.

No precise rules can be offered for determining such possible over-lap. For the component elements, one can say that an overlap in meaning usually exists if one component directly affects another component. The components, being the physical entities of the system, affect each other through the processes, and maybe at times through certain elements external to the system proper--that is, through one of the determiners-- but they should never affect each other directly. The direct relations between component elements are therefore re-examined to determine whether an overlap in meaning exists, and to determine whether a process was omitted from the element listing. As was pointed out earlier, since there is a gray area between the processes and the direct relations, an analyst may decide to retain an intracomponent direct relation rather than to introduce another process element.

Practical experience has shown that an overlap in denotation between determiners is frequent. The determiners connected by direct relations with each other are therefore carefully scrutinized to ascertain if such overlap in meaning exists, or if the elements truly represent different concepts directly related with one another.

33

Intraprocess direct relations are especially frequent, and their existence is by no means a good sign of denotative problems. Still, here too, these direct relations can be used as a guide by noting whether identical groups of component or determiner elements are in direct relation to two or more processes. If this is the case, it is likely that the process elements are not independent in their meaning.

Where overlaps in denotation are found, the affected elements are restated--sometimes by adding new elements, and sometimes by coalescing two elements into one--and the direct relations affecting these new elements are determined.

In all that has been said so far, certain assumptions were made regarding the nature of systems but no assumptions were made in regard to the quantitative model or models that can be used in the analysis. Thus the selection of the elements and the determination of the direct relations were kept as free as was humanly possible from the quantitative evaluation of the predesignated problem. This separation between classificatory and quantitative description is completely foreign to the methods of science, and in this separation the present approach for systems analysis differs decisively from scientific methods. Within science such a separation is not only unfeasible but highly undesirable, since a meaningful scientific statement is a potentially verifiable statement. Thus the scientist attempts to divide his complexes into those facts, elements, operations, or concepts which he can describe quantitatively and verify empirically, and the requirement for ultimate verification determines his selection of facts.

Systems analysts have repeatedly used the same methods for their fact selection. However, it is my opinion that the blind transfer of these methods of fact selection from science to systems analysis is the factor which, more than any other, has produced highly formal analyses which are practically meaningless since they solve too limited an aspect of the predesignated problem.

To avoid this, the present approach attempts to precede the quantitative description with a classificatory description which can consider wider aspects of the predesignated problem and which is (as far as humanly possible) independent of the quantitative description. However, a quantitative description must follow the classificatory description, since the latter is not a complete description in itself. In the following discussion we shall tie the quantitative description to the classificatory description; thus one can argue that the two are not completely independent. This is, of course, a fact, but it is also a fact that in

34

the present approach the requirement for quantitative description has
not dominated or structured the selection of elements or the determina-
tion of direct relations, as would have been the case if we were engaged
in scientific rather than problem-oriented analysis.

We now begin to consider how a system which is expressed through
elements and direct relations can be analyzed and quantitatively described.
In systems analysis we are primarily interested in tracing input varia-
tions and output requirements as processes through the system and in ob-
serving the effects of these processes upon each element affected. The
system can therefore be regarded as a network in which the elements are
the nodes and the direct relations are the links. The analysis of the
system is then the analysis of this network. For the example given in
Step Two the network would look like this:
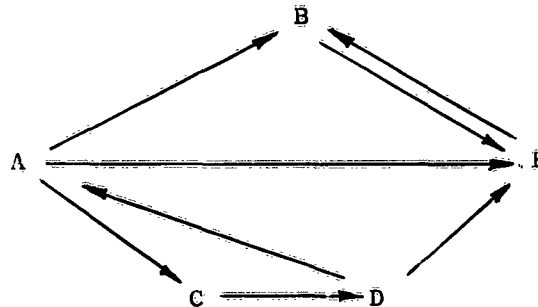
The rigorously formal or mathematical analysis of a network is
theoretically possible if: (1) each link is expressed through an equa-
tion connecting a set of parameters which are the same for all the links
of the network; and (2) if the interconnections of the links at the nodes
follow formal rules. The practical feasibility of the mathematical analy-
sis will be a function of: (1) the complexity and diversity of the rules
which must be followed in connecting the links at the nodes; (2) the com-
plexity of the equations describing the links; and (3) the size of the
network in terms of its nodes and links. If all theoretically possible
links in a network are considered, then the number of calculations to
be performed in the analysis of the network is roughly a function of

35

the cubes of the nodes (n^3). However, if the number of links is considerably less than their theoretical maximum $n(n-1)$, various techniques can be used to reduce the number of calculations required.[*]

If the links and nodes cannot be expressed meaningfully through mathematical analysis, the network will have to be analyzed through judgment. One may ask what type of network an analyst can analyze by means of his judgment, and what factors make this analysis more feasible for him. No criterion can be stated to support an absolute statement that one network is--and another is not--theoretically analyzable by human judgment, and one may wonder what would be meant by such a criterion. What man conceives, he judges, if ever so imperfectly. At least an intuitive answer can be given to the second part of the question. It stands to reason that at least four factors will influence the feasibility or effectiveness with which an analyst can analyze a network through judgment: (1) the complexity of each link, (2) the complexity of the interlink connections, (3) the number of links to be considered in each tracing, and (4) the number of links affecting a node or being affected by a node. These factors are certainly not independent. If we are interested only in one-link tracings, the number of links affecting or being affected by a node is of no consequence. However, if we are interested in tracings of two or more links in length, this number is of great importance; for example, consider the following tracing.

$$A \longrightarrow B \longrightarrow C$$

Even assuming no cyclical nets or alternate paths between A and C, this tracing is incomplete and an oversimplification of the relationship as it occurs in the system, if B is also affected by some other direct relations which in turn affect the direct relation B to C, and if B affects by direct relation other elements besides C, where these direct relationships are affected by the direct relation of A on B. Assuming two additional direct relations affecting B, and two being affected by B, the tracing of A to C is diagramed as follows.

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*   For a specific example, see Klaus Wenke, "On the Analysis of Structural Properties of Large Scale Microeconomic Input-Output Models," Management Sciences, Models and Techniques, Vol. I, Oxford Pergamon Press, 1960, p. 399.

A —————▶ B ————▶ C
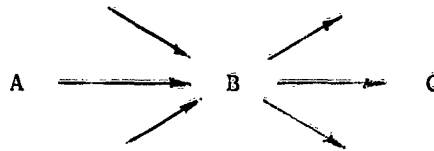
There can be no doubt that the judgmental analysis of this tracing is more difficult than the judgmental analysis of the simpler previous tracing. Indeed, this simple example involved all of the four factors which influence the feasibility of judgmental analysis.

The networks representing systems and system problems of interest to the systems analyst will rarely be of the extreme types which can be meaningfully analyzed in their entirety by means of mathematics, or in which all links and nodes permit only judgmental analysis. In nearly all networks there will be links and nodes which can be analyzed by means of mathematics, and others which cannot be. Therefore our approach must primarily account for these mixed networks.

As we have seen, by whatever mode a network is analyzed, the difficulty of the analysis will be a function of the complexity of the links and the interlink connections at the nodes, as well as of the number of links per node and the number of nodes within the network. These factors are not independent of one another. The complexity of the elements (nodes) will determine the complexity of the direct relations (links) as well as the complexity of the logic required to analyze the compound relations (interlink connections at the nodes). If the system consists of highly complex elements, many elements will be in direct relation to one another and thus the ratio of links to nodes will be relatively high. On the other hand, if the more complex system elements were broken up into a number of simpler elements, the total number of elements to be considered would increase. At first this appears to be undesirable, since the number of nodes (elements) is a factor influencing the complexity (or number of steps required) of the analysis. If the entire network were mathematically analyzable, this increase in elements might outweigh the advantages gained from simpler links and interlink connections, even if the link-to-node ratio is such that certain simplifications can be used in tracing the various processes through the system. In the case of a purely mathematical analysis, the advantages or disadvantages of breaking up complex elements into sets of simple elements will depend on the specific case. However, since we have postulated that there are some links which cannot be meaningfully described through mathematical relationships, the advantages to be gained from representing these systems through simple elements connected by only a few direct relations appear to outweigh the disadvantages incurred from increasing the number of elements.

Operationally, this step is performed by examining each column and row of the matrix constructed for Step Two to ascertain those rows and columns which contain more than a fixed number of "ones," i.e., number of links per node. The elements corresponding to these rows or columns are then examined for possible breakdown. What is the "fixed number?" The number certainly should not exceed man's capacity for reasoning effectively through a network in which that number of links either converges on or diverges from a node. It may be that this is an example of George A. Miller's "magical number seven"--I do not know.[*] However, this number appears to be a fair start. It is also possible that the fixed number is not a function of the number of direct relations converging on or diverging from an element, but rather a function of the sum of these direct relations.

In replacing the elements--which are complex, according to the criterion above--by a number of simple elements, one must note the number of direct relationships which have been added to the analysis. If the number of direct relations added exceeds by a factor of two or more the number of elements added, the revision probably will not simplify the over-all analysis. In this case, the revision may be justifiable only on the basis that the within-element complexity has now been spelled out in greater detail than originally planned, which in turn may or may not be justifiable on the basis of the purpose of the over-all analysis. In short, no arbitrary statement can be made to the effect that those elements should be broken up which have more than the fixed number of direct relations by which they are affected or which they affect. Each case must be examined separately. The final decision must be made on the bases of the over-all purpose of the analysis and the simplifications to be gained by further detailing.

Step Four: Mathematical Modeling of the Elements and Relations

In the first three steps the system was described in terms of elements and relations. In the subsequent steps we shall attempt to explain the functioning of the system in terms of these elements and relations.

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[*] See George A. Miller, "The Magical Number Seven, Plus or Minus Two: Some Limits on Our Capacity for Processing Information," The Psychological Review, Vol. 63, No. 2, March 1956, p. 12.4.1.

From the sciences we have learned that the explanation of phenomena is best accomplished through models. The adequacy (and thus the usefulness) of such models is a function of the degree to which they represent the aspects of the system or phenomena in which we are interested, and the constancy of the explanation which they offer.

The concept of the system in terms of elements and direct relations can be regarded as a model. As a model it can be judged to be highly adequate as a representative tool of the aspects of the system in which we are interested, since this concept can truly and practically contain a large number of highly diversified elements and relations. On the other hand, if an analyst began to analyze complex interrelationships within the system solely on the basis of this concept and his knowledge of the system, he could not be expected to arrive at constant answers as he repeated these operations. In the case of two analysts, this divergence in answers would increase even more. Thus the breakdown of the system into elements and relations must be regarded as an inadequate model from the standpoint of constancy of explanation.

The type of model which is best in its constancy of explanation is the mathematical model. Here the same inputs yield the same outputs (or output distributions, in the case of Monte Carlo models). The drawback of this type of model, however, is that mathematics affords a very limited set of inferences and forces all descriptions into ordered values, while the predesignated problem usually requires the consideration of a wider variety of inferences and value concepts than mathematics can accommodate.

To accommodate this wider variety for consideration in the analysis, we introduced into the approach direct relations on the basis of their existence rather than on the basis of the type of description they require. However, to describe and explain the interaction it is best to use mathematical models wherever possible.

Since we are primarily interested in systems which have relations that are too complex to be described in their entirety through one meaningful mathematical model, the final step in the analysis will have to be a judgmental integration. Thus in Step Four the analysis is not restricted to the development of one over-all mathematical model of the system but is directed toward development of a set of mathematical models which together describe various parts of the system and certain over-all relations within the system. It is expected that in building the mathematical models the analyst will use the network structure of the system as a guide to the aspects of the system which need to be considered; it

is not expected, however, that the models will precisely follow the various network links, but only that the models will parallel the network or certain sectors of the network. Again, since the final integration of the analysis is judgmental, it is not necessary to have no overlap between the various mathematical models. Thus it is quite likely that Model A and Model B will both include certain aspects of the interactions between the ith and jth elements. These aspects will be slightly different for the two models, for if they were exactly alike a formal relation (that of identity) could be established between this particular aspect of the two models, and the two models could be merged into one model.

The end product of Step Four is then a set of mathematical models which together partially describe and explain the system.*

Step Five: Evaluation of the Completeness of the Mathematical Models

In Step Five the mathematical models constructed in Step Four are examined for the completeness with which they represent the system. Since the system network of elements and direct relations is a far more complete representation of the factors affecting the predesignated problem than is the set of mathematical models, the evaluation of the completeness of the models is a systematic judgmental evaluation of the representative aspects of the models against this network.

The evaluation of each model is twofold. First the analyst determines which elements and relations are represented in the model; he then

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* There is a large body of literature on mathematical models, as well as on methods and techniques for their construction, which can be used in systems analysis. Recent publications on these subjects include:

Operations Research and Systems Engineering, edited by Charles D. Flagle, William H. Huggins, and Robert H. Roy, The Johns Hopkins Press, Baltimore, Md., 1960.

Ronald A. Howard, Dynamic Programming and Markov Processes, The Technology Press and John Wiley & Sons, Inc., New York, 1960.

P. Rosenstiehl and A. Ghouila-Houri, Les Choix Economiques: Decisions Sequentielles et Simulation, Dunod, Paris, 1960.

Mihajlo D. Mesarovic, The Control of Multivariable Systems, The Technology Press and John Wiley & Sons, Inc., New York, 1960.

determines the adequacy of this representation. A model is considered
to represent an element if a functional relationship expressed by the
model explicitly contains an operation performed by or upon the element.
A direct relation is considered to be represented by the model if a func-
tional relationship expressed by the model explicitly or implicitly sub-
sumes the direct relation. An element or a direct relation is considered
to be represented adequately if its description in the model contains all
the aspects of the element or direct relation which the analyst judges to
be significant to an analysis of the system. If these twofold evaluations
are made for each model, the end product will show the completeness of
the representation of the system through the models. This type of evalua-
tion is extremely simple if the models precisely follow the network struc-
ture of the system. This will sometimes occur, but the mathematical
models will usually combine elements and direct relations into single
concepts rather than stand in a one-to-one correspondence to the network.
The evaluation procedure must therefore be geared toward handling cases
of this type.

Let us again assume that we have the unusually small and simple
system which was first mentioned in Step Two and for which the network
structure is as follows:

A ──→ B ──→ E ←── A, A ──→ C ──→ D ──→ E (network diagram: A→B, B→E, A→E, A→C, C→D, D→E)
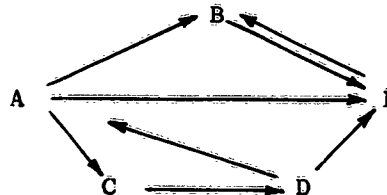
Let us further assume that a mathematical model representing this
structure is the equation:

$$\text{Model 1:} \quad E = f(A)$$

In respect to the structure, this function is obviously incomplete.
The structure includes five elements, the model only two; therefore we
can immediately say that the model does not represent elements B, C,
and D. In respect to the direct relations, the problem is more com-
plicated. We have in the structure the following direct relations:

A ──────→ B
A ──────→ C
A ──────→ E
B ──────→ E
C ──────→ D
D ──────→ A
D ──────→ E
E ──────→ B
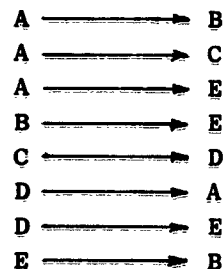
Since the model represents only the relation between A and E, the
first answer appears to be that the model includes only the direct re-
lation A ⟶ E and that all the other direct relations are not represented
by the model. This interpretation may well be erroneous. The structure
implies that there are three paths from A to E, feedback excluded, namely:

$$A \longrightarrow \bar{B} \longrightarrow \bar{E}$$

$$A \longrightarrow E$$

$$A \longrightarrow C \longrightarrow D \longrightarrow \bar{E}$$

In the model these three paths may all be represented in the func-
tional relation of A on E. Thus it is quite likely that the direct re-
lation and the two compound relations are all represented by the model.
Since the middle element of the compound relations is missing from the
model, these relations are certainly no more than partially represented
by the model; furthermore, since the model includes no feedback, the
feedback loops of these compound relations (direct relations D ⟶ A and
E ⟶ B) are certainly not represented by the model.

Without having more knowledge of the system and the precise func-
tions expressed by Model i, only the following conclusions can be drawn:

Conclusion 1.   Model i partially represents the affects* of ele-
                ment A on element E.

Conclusion 2.   Model i does not represent elements B, C, and D,
                nor the affects on A or the affects of E.

Conclusion 3.   Model i does not represent direct relations D ⟶ A
                and E ⟶ B.

Conclusion 4.   Model i does not represent, or represents only par-
                tially, the compound relations A ⟶ B ⟶ E and
                A ⟶ C ⟶ D ⟶ E.

Conclusion 5.   Model i represents direct relation A ⟶ E either
                not at all, only partially, or adequately.

Conclusion 6.   Model i represents at least partially either A ⟶ E,
                or A ⟶ B ⟶ E, or A ⟶ C ⟶ D ⟶ E.

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* The use of this term is discussed on page 27.

The analyst, by his knowledge of the system must make the final
decisions between the choices presented in conclusions 4 and 5. In this
example, we shall assume that the analyst's choices will be:

Conclusion 4. Model i represents only partially the compound
relations

$$A \longrightarrow B \longrightarrow E, \text{ and } A \longrightarrow C \longrightarrow D \longrightarrow E.$$

Conclusion 5. Model i represents direct relation $A \longrightarrow E$ adequately.

With these definitive restatements of conclusions 4 and 5, conclu-
sion 6 is no longer necessary, since it contains no additional information.

Since operationally a matrix scheme has been used to represent the
system in terms of elements and direct relations, this scheme can also
be used in Step Five to record systematically the results of the evalua-
tion of the mathematical models. The results of the evaluation are rep-
resented through judgments on whether certain relations and certain af-
fects on or of elements are either adequately or partially represented
by the various models. An adequate representation can then be symbolized
by a_i and a partial representation by p_i, where the subscript refers to
the model through which the representation is made. Where a direct rela-
tion is adequately or partially represented by the model, the symbol a_i
or p_i is inserted into the cell representing the direct relation. Where
the affects of an element are represented by the model, an a_i or p_i is
placed in the row heading, and if the affects on the element are also
represented, the corresponding symbol is placed in the column heading.

For our example, the results of the completeness of the evaluation
of Model i can be summarized in matrix form as follows:

		A	B	C	D	E
						p_i
A	p_i		p_i	p_i	0	a_1
B		0		0	0	p_i
C		0	0		P_i	0
D			0	0		p_i
E		0		0	0	

43

In this matrix, the 0's again indicate those cells which do not represent direct relations of the system. The matrix summarizes the five conclusions as follows:

Conclusion 1.   Through the p_i's in the subheadings of row A and column E.

Conclusion 2.   Through no marks in the subheadings of rows B, C, D, and E, and columns A, B, C, and D.

Conclusion 3.   Through no marks in the cells representing relationships $D \longrightarrow A$ and $E \longrightarrow B$.

Conclusion 4.   Through the p_i's in the cells representing the relationships $A \longrightarrow B$, $B \longrightarrow E$, $A \longrightarrow C$, $C \longrightarrow D$, and $D \longrightarrow E$.

Conclusion 5.   Through the a_i in the cell representing the relationship $A \longrightarrow E$.

A number of column and row headings, as well as a number of cells, will contain more than one entry at the end of this process. Where these entries consist of a number of p's, the analyst will have to decide whether these partial representations are equivalent to one adequate representation. If the answer is "yes," this is indicated by an appropriate code such as a_s, where the subscript indicates that the adequacy is due to summation rather than to any one model. A row or column heading marked by an a_s would imply that the affects of or on the element are adequately represented by the mathematical model. This in turn can be the case only if all the existing direct relations represented by cells in the row or column are adequately represented. Conversely, if all the direct relations in a row or column are adequately represented, then the row or column heading should contain an a. This relationship, of course, is equally valid regardless of whether we deal with a_i's or a_s's. On the other hand, the fact that a direct relation is partially represented does not imply that the element affecting or affected by the direct relation is represented. A case in point is our example, where it was possible that the direct relation $C \longrightarrow D$ was partially represented (conclusion 4), but where the elements C and D were not represented (conclusion 2).

If several mathematical models are used, and especially if three or more of these overlap, the symbols in some of the individual cells may well become crowded, and the analyst may prefer to replace the p_i's with an a_s and keep the detailed information on another record. It is

of importance, however, that the analyst has a single visual representation which gives an indication of the elements, the direct relations, and the adequacy with which these are represented by the mathematical models.

As Step Five is completed, it is possible that the analyst may become aware of additional aspects of the system or its subsectors which can profitably be represented through mathematical models. Should this be the case, it is expected that these models will be constructed and evaluated by the method outlined above.

Step Six: Description of the Incompletely Modeled Relations

The direct relations were introduced in Step Two on the basis of their existence. At that time no description of these relations was offered. Step Three implied an informal awareness of the description on the part of the analyst, but in Step Four we were for the first time concerned with the description of these relations. However, we restricted ourselves at that point to those relations which could be described through mathematical models. Then in Step Five a determination was made of the adequacy of the descriptions offered by the mathematical models for describing the direct relations. This determination again implied an informal awareness of the description. To obtain a complete and explicit description of the direct relations, which is a necessity for an explanation of the functioning of the system, an attempt must now be made at further description of those direct relations which were not adequately represented by the mathematical models--that is, all direct relations which exist and are not either a_i's or a_s's.

These additional descriptions cannot be accomplished through mathematical models or they would have been performed previously. They will therefore be far less formal in character. In most cases the best descriptions possible will be qualitative judgments of the influence of the affecting element on the affected element. However, forcing the analyst to express these judgments not through feelings or by some hidden thought process but through a linguistic expression--usually a sentence or paragraph, but at times a table or figure--may well assist him to clarify and stabilize his judgments regarding these direct relations. Where the direct relation has been partially described through a model, the full description of the relationship will then be the model description and the additional informal description supplied by the analyst in the present step.

It is physically impossible to include these descriptions in the matrix presentation, but the matrix can be used as an index to the

location where the description of the relations can be found. The matrix
is also used in making sure that all direct relations have been described
in one way or another.

Step Seven: Judgment Integration of the System Analysis

The entire system has now been described, and certain processes
within the system have been explained through mathematical models. All
that remains is to answer the predesignated problems which gave rise to
the system analysis in the first place.

While the predesignated problem is the raison d'etre of the system
analysis, in this approach the problem itself has entered the analysis
so far only at one point--the selection of system elements. Here a con-
scious attempt was made to include all those elements which stood in an
affecting relationship to the predesignated problem. After the elements
were once fully established, no further reference to the predesignated
problem has been made until the present step. The reason for this is
not accidental. Although the predesignated problem should circumscribe
the analyst's field of inquiry, it should not determine his course to
such an extent that he overlooks the side effects and apparently peri-
pheral problems which may have an effect on the predesignated problem
in the long run. Thus everything was done to broaden the analysis so
that it could be more meaningful than an analysis which is dominated by
a clearly defined and often oversimplified measure of effectiveness.

Single measures of effectiveness, however, also have their advan-
tages. It is through these measures that complex system problems can
be focused--that they can in fact become comprehensible. To a decision-
maker, the wealth of data assembled by the end of Step Six may by its
quantity be more confusing than clarifying. A company's profit and loss
statement may not reveal everything about a company, but it brings the
success of the company's recent operation into focus. On the other hand,
if we focus too exclusively on a single factor we may suddenly find our-
selves in serious trouble. Past profits are not necessarily an indicator
of future profits. Thus we must strike a balance between the "too much"
and the "too little." The judgment integration of the systems analysis
should therefore focus the results of the analysis without a confusion
of data.

Within the elements and relations network, focusing can be accom-
plished in at least two ways. The analyst may focus his attention on
any one element or he may focus on a chain of direct relations which
connect two given elements with one another. In either case, he will

46

notice the various elements and direct relations which affect his focus and then broaden his focus as he sees fit.

If the analyst is interested in a particular physical entity of the system, a process within the system, or the relationship of the system to a factor external to the system, he will select an element as his focus. If his interest is, rather, in the relationship of an external factor with a process or component of the system, he will more likely select a chain of direct relations as his focus. In either case, the analyst will proceed in his integration by comparing through informal judgment the model results and other descriptive material which pertain to his particular focus.

While no formal rules can be stated to assist the analyst in the selection of the "proper" focus, his freedom of choice in selecting one focus or a number of focuses is one of the unique features of the approach presented. We noted that predesignated problems are, for all practical purposes, poorly defined problems and problems which do not lend themselves easily to a precise meaningful definition. The proper answer to these predesignated problems is therefore not the answer to one precisely defined problem but rather the answer to a number of these more precisely defined problems. Each focus integration can be regarded as an answer to one of the number of "more precisely defined problems." Since all the focuses originated from the over-all analysis, the total end product will be a set of self-consistent answers, even if these are not neatly wrapped up in a single measure of effectiveness.

Problems of the Approach

In spite of the fact that the matrix-network approach has been often described through methodological rules, the approach is not a method, since it lacks sufficient precision to merit this title. Ultimately, of course, we want a method for systems analysis just as we want and have a scientific method. While the approach may be regarded by those who agree with it as a step in the direction of such a method, there are at least three assumptions which were not analyzed and which require careful analysis if the approach is ever to be developed into a method. These assumptions are that there are predesignated problems, that systems are bounded, and that analysts are knowledgeable and have judgment. While one may be quite willing to accept these statements, they are not simple statements, but rather complex concepts which may well imply different ideas to different people.

What is a problem? Does a problem statement imply a solution--albeit ever so vaguely? If I go to a physician and say, "I have a pain," the solution seems to be to remove the pain. How this solution can be effected is, of course, another question. Thus I seem to go to the physician not to have him find the solution--I already know what this is to be--but to have him tell me how the solution can be effected and possibly to execute the plan he formulates for effecting the solution. On the other hand, we often hear from the analyst that those who request his services don't really know what their problems are. This statement appears to imply that, if the solution is effected which the predesignated statement of the problem implies, then there will be other problems not yet foreseen by the analyst's client. This in turn implies that the analyst's predesignated problem, while including the client-stated problem, is not necessarily limited to it. The present approach accounts for this lack of identity to some degree by regarding the predesignated problem as being shrouded in gray areas, rather than as a well-defined problem.

While this may be regarded as a practical solution, it raises a second question: What are the boundaries of the system that the analyst must consider? The present approach implies essentially two answers: (1) that the physical limits of the system are coextensive with the physical entities that make up the system; and (2) that the analytical boundaries of the system as considered in the analysis also include the factors external to the system which affect the system. Thus the system has, in effect, two sets of boundaries--a physical one and an analytical one. In other writings this duality has been expressed as a concept of the system (as such) and the system within its environment, or as a concept of each system as part of a supersystem. The concept of a hierarchy of systems is widely used in systems analysis. The closest approximation within the present approach to the consideration of a hierarchical structure was in Step Seven, where the problem of focusing the integration was discussed. The general approach here, however, has been to treat the system as a whole, rather than as a multidimensional hierarchy. Still, unless we want to try to understand the whole world in one swoop, or unless we expect to reconstruct it out of its atoms (in the Greek meaning of the word), our understanding divides the world hierarchically. Thus the present approach requires an extension to show how a subsystem analysis is related to a system analysis, and how the latter is related to a supersystem analysis.

There can be little doubt that such an extension requires, first of all, a careful analysis of what we mean and what is implied by the predesignated problem and the boundaries of a system.

48

Whenever, within the present approach, we were unable to formulate a rule or to describe how a given step could be accomplished, the ploy of the knowledgeable analyst's judgment was introduced. He with his judgment was supposed to solve the problem which could not be formulated with sufficient precision to permit a formal answer and thus be expressible in terms of a rule. If judgment is considered to be the logic used where formal logic is not yet applicable, the problem of judgment can be regarded as an unimportant one in itself, since it will ultimately be replaced by a formal logic. On the other hand, if we take the more realistic approach that judgment is introduced where formal logical considerations are not applicable, then judgment is a problem in its own right. If we regard judgment from this latter point of view, an analysis of the contribution which the knowledgeable analyst's judgment makes to systems analysis appears to be one way of attempting to give the present approach the precision required of a method. To lay the foundation for such an analysis, five authors will discuss four different aspects of the problems related to the knowledgeable analyst.

IV THE CASE STUDY AS AN AID TO THE ANALYSIS OF COMPLEX SYSTEMS
by Leonard Wainstein

> Editor's Note: The most complex systems which have
> to date been subjected to systems analysis are those
> involving a great number of human beings acting indi-
> vidually, who have different levels of motivation
> and different objectives. Systems of this type are
> motor vehicle traffic control, city planning, compet-
> itive business situations--and war, especially the
> command and control aspects of military conflict.
> In the study of these very complex systems, it has
> recently become prevalent to begin with a case study
> analysis of a past situation which displayed charac-
> teristics similar to the system to be analyzed. The
> case study is thus really a prestep to a systematic
> systems analysis. In this chapter, Wainstein dis-
> cusses the role and value of such case studies as
> well as their problems and limitations. Wainstein
> also points up the role the analyst plays in the
> performance of these case studies.

Systems analysis concerns itself with specific situations and spe-
cific problems. Its focus is on the unique, not the general. However,
it is unlikely that most problems or parts thereof, however unique, have
never had a counterpart of some sort and to some degree in other specific
situations. The examination of historical counterparts can offer a
fruitful source of insight to the analyst. The methodological technique
is the case study method, and this chapter will attempt to sketch some
of the ways the case study method can be of assistance. The strengths
and limitations of the different types of case study will be examined.

The search for analogies, it must be made clear, has very definite
boundaries and limitations. Parallels are never exact or complete.
Superficial similarities can often blind one to fundamental differences.
Nevertheless, true "uniqueness" is probably hard to find. The systems
analyst with his interest in "unique problem X" will still find it of
value to seek parallels for insights from the analyses and solutions of
earlier counterparts. It should be made clear that the sorts of complex
systems referred to in these pages will be collections of processes,

functions, and concepts which may or may not be associated with specific physical complexes. The case study is most useful in providing insights as to the human part of man-machine system relationships. It is primarily a methodological tool of the social sciences, but its value has also been shown in the more hardware-oriented problems of operations research and systems analysis.

A case study is an attempt to describe and understand a unique event or series of events in toto. Completeness is its distinguishing feature in contrast to the use of only certain information about a past event to test or illuminate a current problem. It is a "description" of something that occurred, of the interrelationships and interactions of components within a certain broader matrix. Yet it is more than a mere collection of facts; it goes beyond plain description to analyze why things happened as they did. Interpretation and assessment are integral stages in a case study. Only by post facto analysis, utilizing our knowledge of the ultimate results and aftereffects of the particular situation or problem can we see all the interworkings of the component elements. Hindsight is not merely permissible, it is indispensable. What distinguishes the case study from a formal and straightforward history is this element of purpose. The case method is designed to provide insights for use outside the compass of the immediate case under study. Its aim is not merely to gain complete information about a particular episode; it is not an end in itself. The case study is only a first step for the analyst. It is, for instance, an input to a system analysis of the type discussed in Chapter III; its lessons must be put to use by the analyst in the analysis of the system of his prime interest. This point of purpose must be constantly kept in mind during the case study in order to achieve results most useful to the broader problem.

It may be argued whether the greatest value of a case study lies in its completeness of description or in the sophistication of the following analysis. The weight values of the two stages differ, as do their reliability. This point will be discussed further later in this chapter.

In the analysis of a complex system, a case study of a similar system operation or part of a similar system provides the analyst with an actual working example. The analyst does not have to imagine one or create one in theory. His own visualization of the specific system he is concerned with is clarified by having at hand a picture of a similar system in operation. In this system he can see the interrelationships that clashed, and especially the unforeseen circumstances that occurred and the factors that had to be met. The case study is both a source of useful data and new ideas and a sounding board against which the analyst can examine other new ideas. The method is a means of generating new concepts and conclusions.

Above all, perhaps, the "real" case study illustrates relationships of cause and effect. The definition of "cause" here implied is admittedly narrow and special, being based upon the fact that a case study is primarily an analysis of purposive human behavior in a given situation. The concept of "cause" is really anthropomorphic. The facts of the physical world do not have a link analogous to that idea of purpose which, in events and operations involving humans, links effects to causes. In human activities the cause is not only the indispensable antecedent but also contains the element of intention which produced the event. A cornice falling from a building and killing a passerby does not involve intention, but a man throwing that cornice does. The case study illustrates the critical role of intention in cause in operations involving humans.

The injections of realism act as constraints which compel the systems analyst to avoid forcing his specific problem to fit any preconceived notions or any pre-established generalized analysis which may offer tempting means of simplification and solution. They tend to fix boundaries to his freedom of choice, both of analysis and solution. The real life factors thus help to ensure that the analyst will fit his analysis to the problem rather than the problem to the analysis.

The case method thus provides a means of "testing" the reality of the analyst's concepts, inputs, and conclusions. "Testing" is probably a dangerous word to use in this context, but it is meant in the sense of a mirror in which the analyst's concepts, inputs, and conclusions can be compared with the real life factors of the case study. In other words, the case method permits at least a partial descent from abstraction to reality. Needless to say, this is not an absolute statement, since systems analysis will always involve some degree of prediction and therefore of abstract analysis. The more "real" inputs one has, the less one will have to rely upon hypothetical creations of unchecked and often uncheckable value.

The case study method is not without its own mechanical methodological problems. The existence of these and their treatment in any specific study will affect the usefulness of that study when its lessons are applied by the analyst to his broader problem.

The first limitation may come in the matter of data availability and reliability. Whether one depends in a case study upon documents or upon interviews, one can never be certain that one has all the pertinent critical details or that the available data are accurate. In the first place, much is never put on paper in the form of records, and secondly, personal memories fade or warp very rapidly. Personal reminiscence is a source to be handled with caution: the problem becomes more difficult

as the case example becomes more remote in time. The dead cannot be analyzed and interviewed, and documentation gets scattered. While improvements in research techniques can improve the matter of data availability and reliability to some extent, there are sharp limits.

Another problem of the case method concerns the matter of the research analyst's interpretation of his case study, which, as we have already stated, is a vital part of the case method. In matters historical, variability of interpretation is always possible and, indeed, perhaps even probable. This variability may be the result of more than merely different interpretations as to the meaning of certain causes and effects. A further element of variability can be introduced by the bias resulting from the researcher's academic disciplinary focus. An economist may interpret the facts of a historical situation in a fashion giving economic factors prime place as historical motivations. The political scientist will do the same for political pressures; the sociologist, social and cultural factors. A given situation may well have had all these factors operative in it, but in the attempt to understand cause and effect, the analyst must recognize that the factors were not likely to have carried equal weight.

These subjective biases may be obvious or they may be subtle. They may be conscious or they may be unconscious. In either case they are inevitable. The systems analyst must keep these caveats in mind when he comes to the point of using the case study as a tool in his larger problem analysis.

The selection of a specific case to study as illustrative of and bearing a similarity to the over-all complex problem under analysis or to a part thereof, can also present a "problem of abundance," which is not quite the reverse of the situation of incompleteness of data described above. This problem can break down into two parts. The first concerns selection of the case study itself. From the mass of likely candidate cases, which one or ones are to be selected? What are the criteria for selection? This is a highly subjective matter and one which rests completely upon the "knowledgeable analyst." Only he will have enough grasp of his main problem (and this must always be the guidepost) to know where and how case studies will be able to assist him, and only he will possess the insight to see similarities between the partially formed facets of his own problem and other similar examples. Exact parallels, of course, are hardly possible, and one might postulate a rule to the effect that case studies should not be undertaken until one has sufficiently examined the scope of his own problem and laid out its main facets. Only then will the analyst have gained sufficient familiarity with his own larger problem to make comparisons with other cases and to draw useful insights from them.

54

The undertaking of case studies too soon can lead to a pursuit of wrong lines of endeavor and to results which do not offer the kinds of insight the systems analyst is seeking.

Obviously, the task of what might be called "adaptation," the drafting of a study from documents or interviews is generic, and is not, as such, specifically a result of the problem of abundance. However, abundance of candidate materials certainly affects the task of adaption. From this large mass of relevant (and irrelevant) cases must be chosen a case study best suited to illustrate and illumine the analyst's own problem. Then the material of the selected case study itself must be chosen with an eye to the analyst's over-all problem. This represents the second part of the methodological problem area.

This is the problem of selection of facts, of what is important. Obviously, the analyst must avoid selecting only such facts as he may find convenient, thereby using the case method to buttress his preconceived ideas about his own broader problems. This misuse of the case method is far from unknown. Since the analyst inevitably will develop certain notions about his own problem at a certain stage in his analysis, the moment in the larger problem study at which case studies are selected and launched must be chosen with care. The moment should be after the analyst has developed sufficiency of grasp of his problem but before his preconceived notions tend to harden.

Part of the problem is the selection of facts, and indeed, even more basic is the question of what is a fact. Any social science or historical research encounters this problem. It has already been mentioned that one can never really be sure that evidence is totally correct or that the deductions drawn from it are sound, since the non-physical science type "fact" is subject to interpretation to a vastly greater degree than its physical science counterpart. There is no need to pursue this problem here. Let it be said simply that records of past human events can never be considered as absolutely accurate beyond the slightest shadow of doubt.

The last point to be made concerns the problem of "uniqueness," which will lead to a further discussion of the differences between two types of case studies of use to the analyst. The problem of uniqueness involves the larger problem of the value of "history," of a study of past events as a source of insight to the present and the future. The case study we have been talking about thus far in this paper is of a one-time event. Alone, it has the value to the analyst we have ascribed to it--it offers an example of what can happen in a problem situation of somewhat the same nature. However, nothing more can be ascribed to it in the way of being

a basis for generalization. It must first be "compared" to other, similar[*] case studies. In short, one conducts a series of case studies of similar problems and is then in a much better and stronger position to attempt to make a generalization which, in turn, provides the analyst with more soundly based insights into his larger problem. One is thus providing a basis for "comparisons" and for a search for "regularities." If, within the contexts of several case studies, the same elements appear and act in certain similar ways, one is on much safer ground in assuming the probability of the presence of that element in any other similar situation. Certainly one acquires a firmer check point on the range of possibilities, which is, in the last analysis, what one attempts to construct from a series of case studies. Again it must be emphasized, however, that terms like "regularities" or "common elements" are purely relative within this context. Exactness will never be found, but at least the analyst is given some idea of what to look for or expect in his own unique problem.

The situation thus presented leads close to a classic problem of historiography, namely, the ability (and right) of historians to "generalize" in the face of the one-time uniqueness of history. History deals with the unique; so does systems analysis. Yet the greatest value of the study of history comes from its insights more broadly based, namely, its generalizations. The sequence in the utilization of history by the systems analyst should go something like this:

History (unique events) ——→ history (broad generalizations)——→ systems analysis (application of insights to specific problems).

or

unique events ————→ generalization ————→ unique problem

The ability of the historian to provide such generalizations is a moot point, especially in the eyes of some social scientists. The development of social science methodology in the last few decades has given rise to a new approach to the case study, one in which the case study can be based upon more quantitative evidence than had ever been available to the historian. A still unresolved debate has ensued between

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* Similarity as used in this paper is a relative term. Since perfect historical parallels are nonexistent, we are dealing with events in which the surrounding circumstances, the motivations, and the dynamic forces have characteristics in common. End results may be very different, but our major concern in a case study is with the dynamics of the situation and why these operated so as to produce a certain result.

historians and social scientists over the merits of their respective methodological approaches and the validity of their subsequent outputs.

The nub of the problem lies in the right to generalize or, carrying it a step further, to predict human behavior in the way the physical sciences predict.

At the same time, the historical method has also tended to form into two schools which, in their own way, resemble the divisions between social science and history. Social science has followed suit, with advocates of each school of more or less rigor in placing more or less emphasis on research and analysis. Since these differences, both internally and between history and social science, affect the value of the case study method to the systems analyst, it may be worthwhile to discuss the nature of these differences and their relevance to the subject of this paper.

First, the schism in social science. David Riesman has said that:

All social science work today establishes itself on a scale whose two ends are theory and data. At one end are the great theoretical structures by which we attempt to understand our age; at the other, the relatively minuscule experiments and data which we collect as practicing social scientists.[*]

In between are smaller schemes of generalization, as well as larger and less precise observations. The relationship of the two ends of the scale is never completely clear.

Attempts to break down the large theories into pieces which can be tested are still unproven, and, to use Riesman's words again, no one has yet developed a general method of "going from the twigs of research to the main trunk of social science theory."

However, the sharpness of the polarity between data and theory schools has varied with time. With the development of the new techniques of testing in the last thirty years, even if only weak tools now, there grew a suspicion of and condescension toward the so-called impressionistic work of early writers, including the most significant works in the development of social science. Very recent years have seen a tendency on the

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* David Riesman, "Some Observations on Social Science Research," The Antioch Review, Vol. II, September 1951, pp. 259-278.

part of social scientists to avoid rigid adoption of one extreme or the other, with a consequent willingness to utilize both inductive and deductive approaches.

At the same time, the problem of the development of the new tools is that they suggest a strict form of social science in which every generalization can be supported by data or by unassailable deduction from secure data, these generalizations then being as useful as those of the natural sciences. However, for the present at least, such generalizations often tend to be hobbled by the very same scientific techniques and social science can become less meaningful as far as permitting us to understand a broad sequence of development.

In view of the difficulty of linking any important generalization to measurable data, the basic problem remains. There is no question that the new methods and techniques of research must be accepted, inelegant and narrow though they may be, but at the same time, the essence of social science should not be downgraded, namely its power to illuminate and describe in some larger framework the experienced details of life. It must be so, for it has been wisely said that in the social sciences we are today in a position where we cannot prove all that we know. As Immanuel Kant put it, "Theory without fact is empty and empirical investigation without theory is blind."

After all, theory itself is only an effort to explain and order facts. Agreement with some large and crucial facts may be more important than contradiction by them in some details. Often, even theories which have been refuted in a formal manner may still offer valuable insights. There are many cases where theories refuted on a basis of small facts still provide useful ways to organize thought. Tawney's investigation of religion and the rise of capitalism is certainly a case in point, or, the map maker's flat earth maps.

Now, a word about the two schools in historiography--the broad and the narrow approaches. Historical facts really begin to acquire significance only when they are grouped in a system of cause and effect. Only then can it be said that knowledge leads to wisdom. In reconstructing the facts of the past, we can set ourselves two different aims. We can limit ourselves to ascertaining facts one by one, or we can ask whether there exists a connection of cause and effect between preceding and succeeding facts. Historical writing which stops at the ascertainment of facts may be erudition but not really history. History is the effort to organize these facts according to the principle of causality. What makes this somewhat different from the regular method of science, however, is that the historian cannot prove cause and effect. He can only interpret.

58

From this the next step is to the development of generalizations, and it is here that one of the greatest differences within the ranks of historians occurs. The school of thought which believes in the need for generalization uses the term in the sense of a proposition that describes some attribute found common to two or more objects or situations. Certainly a generalization isn't much of a generalization if it speaks only of two cases or objects, but it is still something more than a statement of the unique. The usefulness of the generalization is enhanced by the frequency with which the common attributes appear. Historical generalizations can really be made safely only on a basis of high frequency of appearance. The more cases examined, of course, the safer the generalizations are likely to be. If a common attribute appears in four out of five cases, it may be presumed to appear in eight out of ten also, but one is much safer in presuming sixteen out of twenty on the basis of eight similarities found in ten cases.

The historian is always seeking the unique, individual decisions, but his real goal should be the study of general "regularities," in order to arrive at the matrix within which these unique decisions are embedded. Both approaches are needed for fuller understanding. General regularities give us a background and a framework, and yet within the framework there always will be the real one-time decisions of individuals. The historian must continue to examine "uniqueness," because the study of regularities cannot, by its very nature, provide the whole relevant story.

The interrelationship of social science technique and historical method shows up in the concept of regularities. This is a quantitative concept in a field concerned mainly with qualitative data. Like all quantitative operations, however, it is based upon qualitative recognition. Nothing can be counted until it has first been recognized as a certain something. The act of recognition is thus an essential precondition to any judgment of repetition, frequency, or, in other words, regularity, loose or precise. It is the insight of the humanistic historian which provides the sources for the acts of recognition upon which all judgment of regularity depends. Recognition must precede measurements of any sort the historian may undertake in an effort to employ social science techniques.

The traditional role of the humanist is to look qualitatively at what has been termed the "figure-ground relationships," to see some pattern stand out from the background which to the more rigorous mathematical mind may appear only chaos or a mass of detail.

Having examined the divisions within history and social science as regards methodology, let us look briefly at one difference of significance

59

for the analyst in his use of the case study method. One of the major differences between social science and historical methodologies lies in the use of advance categorizations or hypotheses. The classical historical view is that questions should come from the material; they ought not be brought to the material in advance. The historian contends that conceptualizing or structuring the project in advance leads to subjectivity. Some schools of social science are sure that the exact opposite is true. To the neutral it would appear that subjective bias is all but unavoidable either way, although its way of making its influence felt will differ.

It is hard to work by a theory in history, except for vast sweeps of time, in the fashion of a Toynbee. The pure "unique event" approach has been gradually discarded by historians, and the value of generalization, to a greater or lesser degree, recognized fairly widely.

However, these generalizations are usually not intended to become an elaborate theory in the way social science has created its own theories. The prior provision of a theory tends to put the historian into a straight jacket.

Nevertheless, attempts in recent years to apply social science methodological techniques to history have undoubtedly helped to expand the historian's outlook and to stimulate his imagination. These techniques and approaches can suggest new ideas and new areas of inquiry. This is, after all, really the principal function of so-called theory in history. By building imaginary systems and deducing how they might behave, the theorists can suggest new possibilities to the historian in his analysis of why things happened as they did. What the historian must always keep in mind, nevertheless, is that it is difficult to relate fact and theory when the issues being studied were of great moment and were emotionally charged.

Historians almost always state post facto hypotheses because this is the way empirical theory has to develop. What the historian does not and cannot do, which the natural scientist insists upon doing and the social scientist aspires to do is to try the same hypothesis on a new set of situations to see if these will operate the same way. The historian cannot test; he cannot "prove" anything he may suggest in the way of a generalization. He may be able to "predict" the presence of certain elements in future situations on the basis of generalizations derived from the study of regularities, but he cannot predict outcomes for the simple reason that he lacks a controlled environment. His contribution to the systems analyst in his attack on a unique current problem is to give the analyst the benefit of past experience. It is as simple a thought as that. The results of case studies can provide the systems analyst with

an eminence, based upon insights from accumulated experience, from which
he can view his problem more broadly and obtain new and hitherto unsus-
pected vistas.

V THE ANALYST IN LINGUISTIC ANALYSIS
by C. J. Erickson and K. H. Schaeffer

Editor's Note: In Chapter II on science and systems
analysis, four major areas were described in which a
general methodology for systems analysis differs from
a methodology for science. Then, in Chapter III on
the matrix-network approach, a systematic but informal
approach to systems analysis was presented, the infor-
mality arising primarily from the repeated insistence
on the use of the analyst's judgment as a decision-
making criterion. In the present chapter, Erickson
and I investigate the use of judgment in an empirical
science which is making every attempt at rigor. Within
this science--the phonological aspects of linguistics--
we attempt to describe the conditions under which the
scientist's position in linguistics is as central as
the analyst's position in the matrix-network approach
and the conditions under which the linguist's position
is less central. As will be seen, the former occurs
in phonemics, and the latter in phonetics.

Whenever within the matrix-network approach a rule cannot be formu-
lated or one cannot describe how a given step might be accomplished, the
ploy of relying on the knowledgeable analyst's judgment is introduced.
He, with his judgment, is expected to solve those problems which cannot
be formulated with sufficient precision to permit a formal answer and
for which the steps leading to their solution cannot be expressed in
terms of a rule.

In evaluating the logical adequacy of the matrix-network approach,
it is necessary to ask whether this ploy is unique to this approach,
thus presenting a unique weakness, or whether it has its parallel in
other systematic structuring approaches, and is only more explicit in
the matrix-network approach than in other approaches.

To obtain a partial answer to this question, we have investigated
the position of the analyst in linguistics, which is a science but has
methods resembling those of systems analysis.*

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* Linguistics, like psychology, has had a number of schools. (Note the
  contradiction between Trager's and Bonfante's articles in The

Linguistics is what the linguist does. And the linguist, according to Bloch and Trager, is:

> ". . . a scientist whose subject-matter is language, and his task is to analyze and classify the facts of speech, as he hears them uttered by native speakers or as he finds them recorded in writing . . . he is less directly concerned with meanings than with the structure and relation of the linguistic symbols themselves; but the nature of his subject-matter obliges him to pay attention to meanings also. When he has described the facts of speech in such a way as to account for all the utterances used by the members of a social group, his description is what we call the system or the grammar of the language."**

Linguistics thus includes both the selection and the classification of the facts of speech. It involves the structure and relation of linguistic symbols, and finally evolves into a "system"*** or grammar of the language. All these steps have their counterparts in systems analysis, where we select elements, classify them, determine their relations, and finally describe the system being analyzed through mathematical models and a network structure.

The similarity between linguistics and systems analysis does not end here, however. Both subjects have a major problem with which to contend: namely, the use of contextual criteria in the selection of their basic components, which in the case of systems analysis are the elements, and in the case of linguistics, the facts of speech. In the matrix-network approach, element selection is determined by the purpose or problem for which the system is being analyzed, and thus the criterion for element selection is not an intrinsic aspect of the element being selected. Examination of an element in isolation does not aid in determining whether or not the element is to be selected for the analysis,

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Encyclopedia Britannica, 1956 edition, Vol. 14, pp. 162A-163 and Vol 20, pp. 313D-313H). In this paper, however, we shall consider only what Trager calls "American anthropological linguistics" and what Bonfante calls "Bloomfield's mechanistic theory."

** Bernard Bloch and George L. Trager, Outline of Linguistic Analysis, a special publication of the Linguistic Society of America, The Waverley Press Inc., Baltimore, 1942, p. 8.

*** Bloch and Trager's use of the word "system" differs from our use of the term. They define it as "an orderly description of observable features of behavior" (p.5). Their use of the term "system," therefore, is analogous to our use of the terms "model" and "structure."

nor can this selection be made on the basis of an examination of a group of elements. The purpose of the analysis has to be considered before the element selection can be made. In linguistics a similar situation exists. The determination of whether or not a particular fact of speech is distinctive, i.e., significant in the language under investigation, or non-distinctive, i.e., peculiar to the speaker, is something that cannot be made on the basis of the speech sound alone, but requires consideration of the meaning to be conveyed by the sound. Thus, the selection of a fact of speech, like the selection of an element, requires the introduction of contextual criteria, and in the matrix-network approach this introduction entails the use of the knowledgeable analyst's judgment.

As a science, linguistics has as its purpose the establishment and verification of general concepts regarding the empirical phenomena of spoken language by means of the scientific method. While the scientific method differs from the method of systems analysis most dramatically in the selection of facts, the most fundamental difference between the two endeavors is found in their relative emphasis on verification. As discussed in the preceding chapter on "Science and Systems Analysis," verification is central to the scientific method, while it is only of peripheral importance in systems analysis. This lack of emphasis on verification lends itself to employing the knowledgeable analyst's judgment as an integral part of the matrix-network approach, since it is certainly true that if we do not insist that our concepts are verifiable, we likewise do not have to insist that they are established according to rigorous rules. However, is the contrary true? Is it necessary that if our concepts are to be verifiable, they must be established through rigorous rules? If we affirm this statement, then there is no room for the judgment of the knowledgeable analyst in the systematic structuring approach of a science. If, however, the knowledgeable analyst's judgment is a necessary element of the systematic structuring approach of a science, then it is not necessary that the concepts in this science be established through rigorous rules. In turn, if the linguist's judgment is an integral part of the methods of linguistics, then the introduction of the analyst's judgment into the matrix-network approach does not make this approach necessarily any less rigorous than the approach of a science such as linguistics.

In this chapter we will attempt to determine whether the linguist's judgment is an integral part of the methods of linguistics, and to what extent this judgment determines fact selection and verification in linguistics, by examining the methods used in linguistics for determining and classifying the sounds of language.

65

While the analysis of the sounds of language (phonology) is a part
of linguistics, it is not all of it--linguistics also includes at a min-
imum, the analysis of grammatical forms (morphology) and the analysis of
the references of these forms (semantics). Phonology is, however, the
first step in descriptive linguistics and thus it is, at least theoret-
ically, independent of morphology and semantics. Furthermore, phonology
has been regarded as the most rigorous subdivision of linguistics, and
therefore in it the analyst's judgment should occupy the least central
position.

Phonology traditionally includes the study of the articulation of
speech sounds, a subject which is primarily physical and physiological
in nature, and which is omitted here; the classification and description
of speech sounds (phonetics); and the functioning of speech sounds in
language structure (phonemics).

The starting point for any linguistic analysis is a phonetic tran-
scription and analysis of the language as it is spoken.

"A phonetic transcription aims to record as accurately as
possible all features of an utterance or a set of utterances
which the writer can hear and identify in the stream of speech.
The more highly trained the writer is, the more closely his
transcription approximates a complete record of the gross
phonetic facts; but it can never be perfect."*

While a tape recorder may now be used for the initial recording, sooner
or later the transcription must be reduced to symbols on paper so that
the sounds can be classified and arranged in some order. What criteria
are to be employed for making this classification and ordering? The man-
ner in which the sound is produced by the speaker? The characteristics
of the vibrations set up in the listener's ear? The vibrations of the
air molecules in the intervening air space? Linguists have chosen the
physiological features of sound production by the speaker as the crite-
rion for classification. This criterion allows a sorting of sound fea-
tures on the basis of the interplay of the vocal cords, pharynx, uvula,
glottis, tongue, soft palate, hard palate, teeth, and lips in the produc-
tion of a sound. A great many more sounds can be produced by the inter-
action of these variables than can be recorded by the standard English
26-letter alphabet. Consequently, phoneticians have constructed a num-
ber of so-called phonetic alphabets for use in recording sounds on the
basis of the means of their production. The various letters in these

* Bloch and Trager, p. 36.

66

alphabets correspond to the positions and interactions of the speech organs. Perhaps the most commonly used phoentic alphabet is the International Phonetic Alphabet, although it is only one of several. It should be noted, however, that none of these alphabets is sufficient for the task, and the linguist or phonetician often finds it necessary to develop a new symbol to represent a technique for sounding which is peculiar to the language or dialect which he is studying. He is constrained, however, to define this new symbol in terms of the technique employed in producing the sound.

Using the criterion of sound production, Bloch and Trager present a classification of vowel sounds[*] and a symbology for these sounds on the basis of three criteria: the part of the tongue which acts as the articulator (front, central, back), the height to which the tongue is raised (high, lower-high, higher-mid, mean-mid, lower-mid, higher-low, low), and the position of the lips (rounded, unrounded).[**]

This classification yields 42 standard vowel sounds, each of which is indicated by a symbol. If necessary, still finer distinctions can be introduced into the phonetic alphabet through the use of subscripts and superscripts. However, in its basic form of 42 standard vowel sounds, this classification already represents more vowels than have ever been observed in any one language. Since the classification is based on possible tongue movements and tongue and lip positions, rather than on observed sounds, the classification has predictive value. At the time this classification was proposed, the standard vowel sounds in the higher-low-front-rounded position and in the low-front-rounded position had never been observed in any language. About 1950 these sounds were found to be present in a Mongolian dialect, and "it was perfectly easy to identify the sounds when they were heard."[***]

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[*] "A _vowel_ is a sound for whose production the oral passage is unobstructed, so that the air current can flow from the lungs to the lips and beyond without being stopped, without having to squeeze through a narrow constriction, without being deflected from the median line of its channel, and without causing any of the supraglottal organs to vibrate; it is typically, but not necessarily voiced. A _consonant_, conversely, is a sound for whose production the air current is completely stopped by an occlusion of the larynx or the oral passage, or is forced to squeeze through a narrow constriction, or is deflected from the median line of its channel through a lateral opening, or causes one of the supraglottal organs to vibrate." (See Bloch and Trager, p. 18).

[**] Bloch and Trager, pp. 19-22.

[***] Trager, _Encyclopedia Britannica_, (1956) Vol. 14, pp. 162ff.

Consonants are classified similarly according to production by the intersection of such factors as stops, spirants, nasals, laterals, and trills with such factors as the labial, apical, frontal, dorsal, and glottal organs. The latter set of factors can be further subdivided, leading to a total of 65 standard consonant sounds. Other features of sound which can be classified are stress, pitch, quantity, and such voice qualities as nasal twang and whisper. Unfortunately, there is not as much agreement on the classification of these features as on the classification of vowels and consonants.

While phonetics classifies sounds anatomically, phoneticians identify sounds accoustically in the transcription of speech. The practical reasons for using different criteria in classifying and identifying sounds are quite obvious. Phonetics, being a science, strives for an objective and verifiable method of classifying sounds. The different anatomical conditions required for producing varying sounds are, at least theoretically, objectively verifiable, and are therefore conditions which can be used for a scientific classification of sounds. This argument is based on the assumption that distinctively different anatomical conditions produce distinctively different sounds, and conversely, that distinctively different sounds are produced by distinctively different anatomical conditions. Within the scope of the present discussion we shall assume that this assumption has been empirically confirmed. Thus in the ideal, the phonetician's transcription of sounds as they occur under "natural" conditions--that is, unaffected speech--involves identifying the sounds by the anatomical conditions which produce them. The most direct way of doing this, of course, is to measure the anatomical conditions present when sounds are produced and laboratory devices have been developed to do just this.* However, these devices are apparently too cumbersome for use in field work and for the transcription of speech produced in a natural manner. For this reason, phoneticians identify speech sounds primarily by their impressions of the sound of an utterance. This non-objective identification of speech sounds leads, in turn, to the fact that there are discrepancies between the transcriptions (i.e., the interpretations) of any two phoneticians, no matter how well trained.**

While in practice the analyst's judgment is a necessary part of phonetics, in theory the introduction of this judgment into the process

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* Leonard Bloomfield, Language, Henry Holt & Co., Inc., New York, 1933, pp. 75-76.
** Bloch and Trager, p. 36.

of sound identification is one of expediency rather than of logical necessity. In phonetics the analyst's judgment is introduced, not because the linguist is unable to state the logical rules by which sounds are classified and identified, but because he lacks sufficient means for instrumentation and measurement of sound-producing anatomical conditions.

The function of the analyst in phonetics is thus fundamentally different from "the ploy of the knowledgeable analyst" in systems analysis. In phonetics the analyst performs functions which can be operationally defined with such rigor that if instruments were available the functions could be instrumented, while in systems analysis he performs functions for which rigorous rules cannot be established. Thus, while the analyst occupies a logically necessary position in systems analysis, the same is not the case in phonetics.

Phonetic analysis classifies speech sounds by the anatomical conditions which produce them. However, as noted above, not all of these sound classes occur in every language. The phonetic transcription of a language, then, will include only those sound classes which occur in the language transcribed, or the objectively different sounds.

To discover the significant sounds of a language phonetic analysis must be supplemented by phonemic analysis. The objective of phonemic analysis is to classify objectively different sounds, phones, into groups of sounds which are "significantly" different--phonemes. Sounds within a language can be regarded as "significantly" different if the hearer must distinguish between them in order to understand the meaning of the utterance.

Lounsbury describes the principles and techniques of phonemics as follows:

"The principle of phonemics is simple. To be significantly different, two sounds must occur in at least some of the same phonetic environments (else there would not even be the opportunity for contrast), and in these environments the choice between them must depend upon meanings rather than be random (else there still would be no contrast).

"Conversely, two sounds which either (a) do not occur in any of the same phonetic environments, or (b) although occurring in some of the same environments, never relate to different meanings are not significantly different. In the first case the sounds are said to be in 'complementary distribution.' In the latter they are in 'free variation.' Either of these,

69

or any combination of them, is called 'noncontrastive distri-
bution.' In such cases the choice between sound types is
either undetermined or determined by differences in environ-
ment, but not by differences in meaning.

"To be in noncontrastive distribution is a necessary condition
for membership of two sounds in the same phoneme, but it is
generally not regarded as a sufficient condition. (The sounds
(y) and (h) are in complementary distribution in English.) In
addition, some unifying feature is necessary. This may con-
sist of one or more phonetic components (articulatory or acous-
tic) present in all the members of the given phoneme and dis-
tinguishing them from all non-members.

"The technique of phonemics is simply one of applying the above
principles. The first prerequisite is careful observation of
the phonetic facts. A reliable phonemicization cannot be made
from inaccurate phonetic data. Given the data, the second
prerequisite is a careful ordering of those data so as to bring
out the facts of distribution of the sound types. One has to
discover in what phonetic environments each of the sound types
does and may occur in the language under study. Given the
facts of distribution, the rest of the process of phonemiciza-
tion consists in grouping the sound types, according to the
phonemic principle, into contrasting classes, such that each
class comprises non-contrasting sound types sharing a distinc-
tive common feature."*

Although the principle of contrastive distribution seems to be ade-
quate as a technique for determining significant differences, there is
variability in this aspect of linguistics, since the number of phonemes
may vary according to the linguist and his purposes. Thus Voegelin
states:

"Some people ask, 'How many English phonemes are there?' One
of the great difficulties I have in talking to graduate stu-
dents is to try to convince them that the object is not to
find the exact number of phonemes of a given language, as
though God had doled out just so many phonemes for each lan-
guage. The object is to work in various ways and phonemicize;

* Floyd G. Lounsbury, "Field Methods and Techniques in Linguistics,"
  Anthropology Today, ed. A. L. Kroeber, University of Chicago Press,
  Chicago, 1953, pp. 404-405.

depending on how one phonemicizes, one may count more or less phonemes for a given language."*

While there may not be an exact number of phonemes in the English language, linguists using these general techniques usually arrive at a total somewhere near forty. Thus Bloomfield uses 41 phonemes to transcribe "the pronounciation of standard English that prevails in Chicago,"** and Bloch and Trager use 45 phonemes to describe "a generalized version of (the dialect of English) spoken by educated persons in the central Atlantic states, from Maryland through eastern Pennsylvania to New Jersey."***

From the foregoing description of phonemics, two facts become apparent. First, phonemes are the contrasting sounds of a language which, if properly related to one another, are the transmitters of meaning. Second, a phoneme is not an intrinsic aspect of a group of sounds so designated but is rather a construct for the purpose of the analysis, since the examination of a group of sounds in isolation will never reveal whether or not the group can be classified as a phoneme.

Within the present discussion, then, the central question is whether the analyst's judgment is necessary in the selection of the phonemes of a language, or whether the position of the analyst in phonemics is analogous to his position in phonetics. The latter will be the case if one can, at least theoretically, formulate rigorous rules for the selection of the phonemes; the former will be the case if such rules cannot be formulated.

In the phonemic analysis of a language, the sounds of a language are grouped into a set of classes. The purpose of this grouping is to have each class represent a contrasting sound (phoneme) of the language.

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  * Voegelin, during the discussion period in a symposium on "Pattern in Biology, Linguistics, and Culture." The transcription of the symposium is published in An Appraisal of Anthropology Today, ed. Sol Tax, Loren Eisley, Irving House, and Carl Voegelin, University of Chicago Press, Chicago, 1952, pp. 299-321.
 ** Bloomfield, p. 91.
*** Bloch and Trager, p. 47.

If we pay no attention to this purpose, the number of ways (T_s) into which the sounds (s) of a language can be grouped can be calculated from the recursion formula.*

$$T_s = \sum_{k=0}^{s-1} \binom{s-1}{k} T_k, \text{ where}$$

$\binom{s-1}{k}$ is the binomial coefficient $\dfrac{(s-1)!}{k!((s-1)-k)!}$

For as few distinguishable sounds as 20, this formula yields a value of $T_{20} = 4.75 \times 10^{14}$, and it appears that for each additional sound the number of ways into which the sounds can be grouped increases by a factor of about 10.

However, as noted above, phoneticians still have difficulty in classifying stress, pitch, quantity, and such qualities as nasal twang and whisper. Because of these uncertainties, the T must be considered indefinite if not infinite in number, unless one insists that the objectively distinguishable sounds (s) of a language have been perfectly defined.

The rules for selecting the phonemes must thus allow for selection of the one set or sets among all possible sets, which best or adequately describe the significant sounds of the language. These rules, therefore, require criteria for "best," for "adequate," and for "significant," and must contain a procedure by which one can establish and examine each and every possible set. The ease with which criteria for "best," "adequate," and "significant," can be developed is dependent on the complexity of the purpose of the analysis. If the purpose can be clearly--that is, unambigously--defined, it is probably possible to state all the criteria involved in the concepts "best," "adequate," and "significant." However, the purpose of a phonemic analysis is usually not that well defined. For instance, does it attempt to discover the significant sounds in the phonetic transcription of simple phrases with unequivocal meaning, or of complex sentences with abstract meanings and with double meanings? To establish measures of "significant" for the former is certainly considerably easier than for the latter. Inflections are probably of little

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* This formula was derived for us by F. W. Doesch and D. A. D'Esopo of Stanford Research Institute.

significance in the former while they may be of significant difference
in the latter. Does phonemic analysis attempt to discover the signifi-
cant sounds in the phonetic transcriptions being analyzed, or does it
attempt to find the significant sounds in these transcriptions so that
the results can be generalized to other samples of the language commu-
nity? To establish measures of "adequate" is easier for the former than
the latter, since the concept of "speech community" is not free from am-
biguity. For instance, when is an utterance an utterance of a given
speech community, and when is it an utterance of a neighboring group or
the private utterance of a sub-group within the speech community? In
both cases, if the purpose of phonemic analysis is the simpler rather
than the more complex one, the results of the analysis will yield few or
no generalizable facts, and according to Poincaré these are not the facts
to which a scientist should devote himself. If the purpose is the more
complex one, complete definitions of "best," "adequate," and "signifi-
cant" cannot be formulated. However, the fact that complete definitions
cannot be formulated does not imply that no definitions can be formulated.
We have partial definitions for "adequate" and "significant" or we could
never prefer one grouping over another grouping. On the other hand, if
complete definitions could be formulated, there should be an answer to
the question, "How many English phonemes are there?"

Because of these definitional difficulties it is impossible to de-
velop a rigorous procedure for examining each and every possible set of
sound groups. If the number of these sets were a known, finite number
a procedure could be developed. Since the number is either indefinite
or infinite the procedure must rely on sampling the possible sets, and
this sample must be sequential, i.e., in the sample the selection of
each set, except the first one, must be dependent on the results obtained
from examining all the preceding sets. Since these "results" involve
the definitional difficulties indicated above, complete rules cannot be
developed for proceding from the examination of one set to the selection
of the next set, and the partial rules that can be formulated must be
supplemented by the analyst's judgment.

The position of the analyst in phonemic description is therefore
not analogous to the position of his colleague in phonetic transcription.
It is analogous, however, to the position of the analyst in the matrix-
network approach to systems analysis.

VI   DECISION-MAKING BY THE KNOWLEDGEABLE ANALYST
by Maurice Rappaport

Editor's Note: In the preceding two chapters we
discussed some logical and methodological aspects
surrounding the concept of the knowledgeable ana-
lyst. We are now turning to the psychological
aspects surrounding this concept. In the present
chapter, Rappaport discusses the psychological
milieu in which the knowledgeable analyst operates.
After identifying the steps involved in the making
of a decision, he addresses himself specifically
to the psychological factors involved in finding
and selecting a problem, structuring the problem
informally and formally, and purposive problem-
solving. While Rappaport does not arrive at a
definitive explanation of these psychological fac-
tors, he shows clearly the difficulties involved
in arriving at such an explanation.

An understanding of the decision-making process by a systems analyst
with sound judgment--the knowledgeable analyst--must come from an examin-
ation of the psychological milieu in which he operates.

Preceding and during the development of any complex system, many
decisions must be made. Some of these can be based upon logic, deduction,
and precise information, while others have to be based upon intuition and
information that is not precise.

No matter what the basis of these decisions, there is a fundamental
need for a way of classifying parts of the system and for specifying the
relationships that exist between these parts or between a part and the
total system. In many instances where there is neither formula nor fact
to provide guidance, a knowledgeable systems analyst must enter the pic-
ture and make decisions about which parts are important by themselves or
by virtue of their interaction with other elements of the system.

With increased understanding of the forces that come into play when
a systems analyst makes decisions that affect the configuration of a com-
plex system, it should be possible to design better systems more effi-
ciently. The aim of this chapter, then, is to focus attention upon
certain psychological processes associated with decision-making. This

is being done to improve understanding of these processes so that we will be in a favorable position to consider means whereby improvements in decision-making activities can be accomplished.

Decision-making as used here refers to the selection, from among many courses of action, of the one or several courses that offer the best solution to a problem. Some problems exist in a highly unstructured context where there is a relatively large number of courses of action that can be chosen, while other problems exist in a highly structured context where there is a relatively small number of courses of action that can be chosen. Most of what will be said applies to both of these situations; emphasis, however, will be placed on psychological processes underlying decision-making in highly unstructured contexts.

A systems analyst approaches a predesignated problem with an uncertain idea of the extent and depth of the subject matter with which he must deal. For example, all he may know at the beginning are the performance requirements that the system to be developed must meet. At first, he will attempt come conceptual representation of the system which will lead to decisions about such things as the configuration of the system and the functions that should be allocated to men and equipment. Ideally, his conceptual representation of the system should also enable him to see critical interactions among the various components of the system (machine-machine, man-man, and man-machine), as well as to avoid pitfalls which would predispose the system to fail.

To help accomplish these and related goals the systems analyst must define as clearly as he can the concepts and terms which he will employ and the rules by which he will carry out his analytical operations. If he can do this effectively he is in a favorable position for deciding which alternative course or courses of action should be pursued.

It is at this point that the psychological processes of the analyst are in ferment and become of interest. The ingredients of this ferment, in quality and quantity, depend directly upon the training, experience, and knowledge of the analyst. The outcome of this ferment depends upon the motivation, the perception, and the various skills of the analyst. The outcome also depends upon the environment and the conditions under which the analyst must work. More will be said about these factors after we consider certain fundamental steps in the decision-making process.

Making a decision involves at least the following steps: (1) finding and selecting a problem through the unique processing of information by the individual, (2) structuring the problem initially through the unique organization of the information that has been processed, (3) teleological or inductive restructuring of the problem to provide direction

76

for problem-solving activities, (4) structuring of the problem so that an acceptable method of analysis can be applied to establish whether or not one or more anticipated solutions can be accepted or rejected, and (5) structuring new problems by using a selective organization of information derived from Step Four.

Steps Four and Five will not be discussed here. Step Four is concerned primarily with the detailed structuring of investigation, viz., the designing of experiments, and this has been handled many times by others.[*] Step Five is part of the feedback, iterative process that characterizes most test, experimental, and research procedures, and it also has been adequately discussed. Steps One, Two, and Three are taken up in the following paragraphs.

1. Finding and Selecting a Problem

A problem may arise in several ways. It may be assigned to the analyst or it may be actively sought by him. No matter how it occurs, it cannot arise out of a vacuum.

Motivation is of primary importance. Without this driving and energizing force, problems would be neither sought nor solved. The source of motivation may be something either external or internal to the analyst. Neither source of motivation is completely independent of the other, but it is convenient in this discussion to treat them as two "types" of motivation. An external motivation source may simply be the requirement to design, develop, and construct a system that can meet certain specifications. Internal motivation involves the development of a self-generated challenge. For example, it may involve re-asking questions about one's ability to handle successfully a large number of details, their integration, and their organization. It also may include re-asking questions about one's ability to seek out and understand essential and relevant information without which it would be difficult or impossible to build a specific system. The individual, in other words, is motivated to maintain his status as a problem-solver and decision-maker and also his image as an intelligent and sophisticated professional. As a result he must begin by making an attack on the problem of finding a relevant problem which he can handle in a satisfactory way. In other words, he must specify a problem he can solve.

* See for example, A. Chapanis, Research Techniques in Human Engineering. John Hopkins Press, Baltimore, Md. 1959.

mental posture with which to examine and evaluate continually other
important and relevant problems. Thus, what is needed for effective
systems analysis is some way of overcoming or reducing normal psycho-
logical inertia and conceptual confinement so that meaningful problems
can be selected. There are several ways in which this may be accom-
plished.

One way is to use competition. This competition may be intraper-
sonal or interpersonal. In the intrapersonal competitive situation,
the analyst must select two or more problem areas that are approximately
equal in importance and he must produce two or more paths by way of
which each problem may be solved. The solutions selected should be ap-
proximately equal in efficiency and practicability. In the interper-
sonal situation two or more individuals may each be asked to select an
important problem area and then to provide efficient and practical solu-
tions to each problem. In either case, obtaining two or more possible
solutions for each problem would provide an analytical situation for
which the final resolution would depend more upon a critical weighing
of alternatives involved than upon personal predilection.

This approach to problem identification and problem solution has
all the advantages and disadvantages of any redundant operation. It
leads, for example, to increased flexibility and reliability in system
planning, development, and construction, but, of course, with penalties
likely in cost and time. Nevertheless, such trade-offs may be essential
if the outputs of system analysts are to be improved.

Another way of enhancing decision-making capabilities is to provide
for the pooling of experience. A systems analyst makes better decisions
if he draws upon experience which extends beyond what he has acquired
via the limited situations to which he has been exposed. Interaction
with other specialists has the advantage of increasing the quality of
decision-making. This comes about, in part, through social mechanisms
which cause the decision-maker to seek confirmation and approval of his
peers in related areas of specialization, thereby obtaining some feed-
back on the correctness, or at least the acceptability, of his decisions.
This approach, however, can be overworked. An analyst who always molds
his decisions according to group pressures is a bad risk decision-maker.

Another bad risk decision-maker is the analyst who seldom or never
sees his decisions result in a successful outcome. The implication of
all this is that a good systems analyst must be a certain type of person
with special psychological attributes and background experience. He
must have a fair amount of success in decision-making, but his outlook
should have been tempered by sufficient numbers of unsuccessful system

analytic efforts so that he demonstrates a cautious and sophisticated approach to complex decision-making problems. He should be predisposed to seek consultation and to pool information but not overly prone to accept all that is proffered. In a word, he should display independent judgment but yet be able to assimilate and selectively integrate into his planning diverse, specialized information.

Yet even these characteristics are not sufficient to satisfy the requirement that a systems analyst isolate relevant problems. In order to work effectively in complex situations, the analyst must be guided by principles that allow him to deal with numerous specific interrelated problems. It is undoubtedly true that at this time no adequate body of principles or generalizations exist for the systems analyst. Thus he must resort to and develop appropriate models. However, while models incorporate reason and may be consistent, this is no guarantee that they are either effective or practical. To be truly effective and practical they must, when they apply to real world systems, have real world validity. This means that any systems analyst must be prepared to back up his model with appropriate techniques of experimentation, measurement, and the utilization of symbolic logic, in one form or another. If he is unable to do this he will be deficient, for there will be no device for self-correction. The analyst could only perpetuate untrue or incompletely true notions that he arrived at in the past. Many of these may be blind alleys of logic and thinking. To employ these without the safeguards of experimental or empirical checks obviously would be a disservice to the system on which he was working. It would also be a disservice to the analyst since he would fail to increase his fund of pertinent knowledge and thus the skill that he brings to successive problems.

These factors have obvious implications for the first step of the decision-making process: the finding and selection of a problem. The identification of this problem depends heavily upon what the systems analyst brings to the situation--his attitudes, his background, his models, and his view of the world, as well as his understanding of the external demands being placed upon him. Once these factors have been used to find and to filter information coming from the predesignated problem area into some meaningful form for the analyst, he is ready for the next step.

2. Initial Structuring of a Problem

It is evident that a problem must be structured in some way before it is possible to solve it. Both the structuring and solution of a problem reflect the operation of such fundamental psychological processes as learning, perception, and motivation--which comprise part of the higher

process called thinking. Much that is pertinent in the thinking process depends upon the individual analyst developing a skill in handling various concepts symbolically. It has been demonstrated that there is a gradation in the acquisition of this skill. For example, Kendler and Kendler[*] have shown that there is a gradation in concept manipulation and problem-solving skills from infrahuman levels to the early levels of growth and development. They show this by using reversal and non-reversal shifts in a simple concept learning task. In this task children were presented with stimuli that varied in two dimensions--size and brightness. The subjects were rewarded for responding to the size dimension (responding to a large cup was considered a positive response; responding to a small cup was considered a negative response). The brightness dimension was made irrelevant. "After learning the first discrimination, the subject [was] forced to shift to another response. In a reversal shift the subject [was] required to respond to the same dimension on which he was originally trained, but his overt choice had to be reversed, e.g., he had to shift from a large cup to a small one. For a non-reversal shift the previously irrelevant dimension became relevant, e.g., black became positive after large had been positive." Rats find a non-reversal shift easier than a reversal shift. College students are reported to execute a reversal shift more rapidly than a non-reversal shift. Slow children about four years of age were more similar to rats in their performance. Fast four-year-olds responded like college students. This transition in problem-solving behavior can be explained rationally by assuming that an increase in skill in verbal and conceptual manipulation has taken place. There is every reason to believe that refinements of problem-solving skills continue into adult years, although specific and objective changes in these skills, their detection, and the factors that influence these changes have yet to be adequately explored. Once relevant factors associated with these changes are better understood they may be employed profitably to enhance conceptualization and decision-making skill of systems analysts. In the meantime, however, since the development and maturation of a systems analyst cannot be adequately delineated within the present state of knowledge, understanding must be sought from another direction. It will be helpful to consider how individuals who function as systems analysts go about giving an initial conceptual structure to a problem selected for analysis.

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* H. Kendler & T. Kendler, "Vertical and Horizontal Processes in Problem Solving," Psychological Review, January 1962, Vol. 69, pp. 1-16.

Informal Approach

The systems analyst may use an informal or formal approach to structuring a problem. If he uses an informal approach, he may turn the problem into a question and let his experience, free association, and intuition suggest "key" factors deserving detailed analysis. Obviously in doing so, he must draw primarily upon his background, although to maintain contact with reality and prevailing accepted analytical approaches, he will seek to obtain feedback and approval from other contributors or he will alter his course to take into consideration changes suggested by them.

In structuring a relatively unstructured problem, the systems analyst is placed in a highly uncertain situation, one in which he is forced to use his creative ability and ingenuity. If we are ever to be in a position to enhance the analyst's ability or ingenuity, we must have some understanding of the nature of the processes underlying these activities. Many different interpretations, however, have been given of the creative process.

A recent arti le by Mednick[*] describes the creative process by leaning heavily on the theory of association. He points to three ways of structuring or achieving a creative solution. He states that ". . . any condition or state of the organism which will tend to bring the requisite associative elements into ideational contiguity will increase the probability and speed of a creative solution." The three ways of reaching a creative solution which he mentions are serendipity, similarity, and mediation. Serendipity refers to the accidental contiguity of events. For example, Mednick cites the discoveries of X-rays and penicillin. He goes on to describe how one physicist has reduced serendipity to a method by placing in a fishbowl a large number of slips of paper, each inscribed with a physical fact. He then randomly draws pairs of facts looking for new and useful combinations.

He feels similarity contributes to creativity in the areas of creative writing, rhyming, and rhythm of words, and also in "domains of creative effort which are less directly dependent upon the manipulation of symbols" (viz., painting, sculpture, musical composition and poetry.)

The third way of achieving a creative solution Mednick calls mediation, a "means of bringing . . . associative elements into contiguity with each other," usually through symbols such as verbal, mathematical,

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* S. A. Mednick,"The Associative Basis of the Creative Process." Psychological Review 1962, Vol. 69, pp. 220-232.

or chemical symbols. He feels, apparently, that creativity is related to the number of associations an individual has, particularly, the individual's ability to form associations between two or more remote ideational elements. The process by which an individual is able to link up remote ideational elements is mediation. He gives the example of three words, rat-blue-cottage, to which a subject must add a fourth word to link them all together. Here, the word is cheese (rat-cheese, blue-cheese, cottage-cheese).

From Mednick's writings it would appear that creative solutions lurk in the ability of an individual to come up with mediating responses to bring together relatively remote facts, ideas, or responses. One of the prerequisites for creativity, however, is a fund of knowledge or a response repertoire that will provide the basic ingredients for creativity. A systems analyst who does not know the elements or response characteristics of his system cannot conduct a systems analysis in a creative way. Without this information he would, to say the least, be hard put to develop a meaningful conceptual structural framework.

Interesting as Mednick's notions are, it should not be thought that concepts of associationism are either new or go unchallenged. They go back at least as far as Aristotle and have had frequent resurgence. Thomas Hobbes,[*] for example, referred to the Principle of Contiguity. Hume[**] in 1739 spoke of resemblance, contiguity in time and place, and cause and effect. In 1829 James Mill[***] spoke of one fundamental law of contiguity. Associationism reached its height about mid-nineteenth century. Reaction to it came from the Würzburg group and Gestalt psychologists, among others.[†] The Würzburgers, notably Kulpe, believed in imageless thought. Instead of associationism, per se, the Würzburgers spoke of "reproductive tendencies" which received direction from Aufgabe--a previously accepted task or mental set. Other indivudals later, like Hovland, considered associationism by itself inadequate for explaining thinking and felt that it has to be supplemented by other concepts such as motivation. Gestalt psychologists, on the other hand, developed the theory of the unitary system under stress and rejected the term associationism[‡] on the grounds that it was to atomistic. We cannot completely

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* T. Hobbes, Humane Nature, or the Fundamental Elements of Policie, Chapter IV, reprinted from Rand, 1912.
** D. Hume, A Treatise on Human Nature, 1739.
*** J. Mill, Association of Ideas, Vol. I, 1869, pp. 56.
 † For detailed discussion of associationism and reactions to it, see G. Humphrey, Thinking: An Introduction to Its Experimental Psychology, John Wiley & Sons, New York, 1951.
 ‡ Koffka, Psychological Bulletin, 1922, Vol. 19, pp. 531-85.

review the history of associationism here, but it is interesting to note that Mednick's article represents another swing of the theoretical pendulum involving thinking emphasizing the importance of associationism in this process.

Formal Approach

If the analyst uses a formal approach to structuring a problem for subsequent detailed analyses, several choices are available to him. These are mostly in the form of models. There are relatively simple and precise templates such as mathematical models. Unfortunately, mathematical models cannot readily deal with complex systems. Even the most complicated models are severely restricted by the number of parameters that they can adequately handle simultaneously. Thus, systems should be represented by mathematical models only when simple abstractions of the system suffice for analysis.

When systems are considered in toto, they cannot be represented completely by any model. If they could, the model and the system would be equivalent. Such equivalence is neither usually needed nor desirable, yet there is a need frequently to represent a complex system fairly closely. In this case, rigorously related symbols are sparingly used. This usually leads to good face-value representations, but poor predictive or manipulative precision, since there is little control over many variables that interact with each other and affect over-all system performance.

Despite this disadvantage of limited control, it is often desirable for the systems analyst to have a comprehensive overview of the system he is analyzing. Such an overview provides a better understanding of how various parts of the system stand in relation to each other, and this understanding, in turn, minimizes the chance that an analyst will suggest unwise changes in a single part of the system in which he may have special interest at the expense, perhaps, of over-all system performance.

Nevertheless, while it is undoubtedly desirable to be able to obtain a comprehensive overview of a system, there are difficulties in developing a method for achieving this.

Since formal models are incomplete representations, their inadequacies must be overcome by human judgment. But human judgment is a nebulous entity. Yet, if the usefulness of a particular method depends upon the application of human judgment, then it is important to examine certain aspects of judgment to determine how well and with what reliability it can be employed. And if judgment is not particularly reliable, it is

important to determine if it still can be used to advantage in structuring systems and conducting systems analysis using one or more of the various qualitative or quasi-quantitative models that are available.

As an aid in examining this question we take the matrix-network approach presented in Chapter III, and primarily Step Two of this approach—the preliminary determination of the existence of direct relations between element pairs. Here rigid logic and precise models must be put aside and the psychological vagaries of human judgment must be brought into the picture by the systems analyst.

Factors that contribute to these vagaries and the mechanisms that are involved in human judgment are discussed in a broader context under "Purposive Problem-Solving," below. At this point, however, to illustrate some of the human judgment problems involved in using a method for conducting a systems analysis, the model referred to above will be the focus of interest.

Both Shapero[*] and Schaeffer[**] indicate that in their approach to the analysis of complex systems, the analyst should deal with direct relationships. Operationally, a direct relationship is defined by Schaeffer as follows: "Element A is said to be in direct relation to element B if a change in A affects a change in B without <u>necessarily</u> affecting any change in any other element of the system, unless such a change, in turn, is affected by a change in B."

It is very well to employ an operational definition to say where A affects or does not affect B. The problem is not in the logic of the definition but in the realistic implementation of the logic. A direct relationship must be "seen" by the analyst. But what is involved when the analyst "sees" a relationship? Can this relationship be "seen" consistently either by the same analyst on two different occasions or by two or more analysts on one occasion? In other words, what is the extent of the reliability of the operation where pairs of elements that have a direct relation with each other are identified?

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* A. Shapero and C. Bates, Jr., <u>A Method for Performing Human Engineering Analysis of Weapon Systems</u>, WADL Tech. Report 59-784, Aerospace Medical Lab., Wright Air Development Center, Wright-Patterson Air Force Base, Ohio, September 1959.

** K. H. Schaeffer, <u>The Logic of an Approach to the Analysis of Complex Systems</u>, Air Force Tech. Report AFOSR 2136, April 1962.

In examining the vagaries of psychological processes that may lead to inconsistent performance from analyst to analyst, several questions should be kept in mind. If we cannot adequately account for the vagaries of psychological processes that contribute to unreliability in performance of system analysts, can a systems analysis approach calling for the employment of such processes still be useful? And if it can be shown that it may be useful despite a certain amount of unreliability, is the cost of this usefulness in terms of time, money, and effort tolerable? These are questions we should keep in mind as various systems analysis models are examined.

To help explore the problem, imagine that element A is a system component. In one instance, let component A be a human operation such as checking a line voltage. Imagine that B is another system component, say an electronic operation which arms a warhead.

With A and B defined in this way, let us ask, "Does A directly affect B?" and then proceed to examine problems that might confront the analyst as he tries to determine this. Does the operation component A, the checking of a particular line voltage, directly affect system component B, the electronic operation which arms a warhead? How can the analyst decide whether the answer is yes or no?

It is clear that the answer may be either yes or no, depending upon the point of view of the analyst and his predilections. If he wants to analyze the system in a horizontal fashion--at just one level--he must structure the system in one way. If he wants to analyze the system vertically--at several levels--he must structure the system another way. Not only does the penchant for structuring a problem one way or the other influence the answer but so does the uncertainty that exists in the interpretation of a specified operation. This uncertainty remains as long as rigid rules for identifying operations or applying a method of analysis cannot be prescribed.

By way of illustrating certain aspects of this problem, let us pursue the above example. Does the human operation of checking a line voltage have a "direct affect" on the electronic operation which arms a warhead? The question quickly leads to a quibble. One analyst might say the line voltage directly affects only the arming operation, not the checking of the voltage. Another might point out that if the line voltage is off by a certain amount and it is not checked, then not carrying out the checking operation would indeed directly affect the occurrence or non-occurrence of the arming operation. Who would be right? Both could be right. It is a matter of interpretation. Wherever equivocation is possible, wherever uncertainty exists, consistency or reliability of response will suffer.

If it was elected to structure the analysis of the system horizontally, there would be further equivocation. Each analyst would have to make interpretations about what constituted components or operations at the same level. Is the checking of a line voltage an operation that is at the same level as the closing of a relay switch, or the passage of a current through a single wire or multiple wires in a cable, or the adjustment of calibration controls? Once more there is room for quibbling and uncertainty and therefore different responses from different analysts.

If it was elected to structure the analysis vertically, there would be even more reason for equivocation. How, in this case, could one say whether or not A directly affects B? In a vertical analysis, it might be argued that a line voltage could never affect an electronic arming operation directly because a line voltage really refers to the organization of electrons in solid matter in such a way that certain submicroscopic physical changes take place and that it is these physical changes that yield a potential difference that directly affects the arming operation and not the line voltage per se. This is, of course, a further quibble. But can one doubt that those interested in the whole spectrum from the macro to the micro will see direct relationships that are different from those who focus solely upon a horizontal level of analysis?

It is easy to see, therefore, that there are many ways of structuring a situation initially, particularly within an approach which strives to be comprehensive. It is well to have an overview, but it should be kept in mind that any single method for achieving an overview of an entire complex has its own variety of slant angles and perspectives. Therefore, we cannot really hope to achieve an optimum method of performing a systems analysis. We can only hope to achieve some method that leads us to adequate solutions to systems analysis problems. Aids to thinking and problem-solving are what should be sought. Any aid to thinking should allow recognition of gaps in our knowledge so that these gaps can be dealt with and evaluated, or, at any rate, not be missed.

The method of dealing with these gaps, however, cannot be prescribed. Their filling in must depend, as was pointed out earlier, upon the motivations, perceptions, training, associations, and other historical accidents that have gone to make up the individual analyst. The nature of complex problems is such that we must always expect unreliability in analysts because of the vast differences in their backgrounds and in their approach to problems. Probably we will rarely ever have the knowledge to predict the extent of this unreliability. Consequently, we seldom will have the opportunity a priori to judge precisely what the usefulness is of a model or method for conducting a systems analysis on

complex systems. The only recourse left open is to try out empirically some of these models, provided they seem reasonable, and see whether or not they merit further investment of time, energy, and money. If they help us structure a system in a useful way, they can be said to be serving us profitably. However, this is so only if the time and cost penalties associated with the employment of these models do not become excessive.

3. Purposive Problem-Solving

Perhaps the one underlying denominator common to all attempts at structuring the world about us and which is worthy of special consideration is the psychological phenomenon of purpose. Note, for instance, that in the matrix-network approach the purpose of the analysis determines the identification, selection, and classification of the system elements.

Science, generally speaking, shuns teleology. Yet teleological reasoning, when applied to the understanding of organismic behavior, particularly man's, has justification. Teleology, it will be recalled, is the concept that a process is directed toward an end or shaped by a purpose. In many instances, man's teleological behavior is a result of his inductive prowess, in other words, his ability to set up hypotheses. Man is almost always hypothesizing. Yet hypothesizing does not always reflect scientific endeavor, although those inclined to scientific pursuits can make good use of this natural predisposition. From a psychological point of view, hypothesizing is frequently purposive behavior which serves a number of needs of the individual. It provides among other things tentative goals toward which he may strive. Much in the same way that the human eye finds it difficult to move in a regular pattern without some outside stimulus to provide direction and guidance, so, too, the psyche and the intellect find it difficult to move in some specific direction without some outside and distant goal at which to aim. This distant goal with all its uncertainty induces psychic and intellectual stability and orientation. It reduces floundering, disorientation, and even anxiety. It gives to the individual confronted with the task of finding solutions to relatively unstructured problems a means to approach and study those problems which is direct, efficient, and unwavering. It, of course, provides no guarantee that the approach selected is the right one or even the most efficient one. But it does permit probes into problems which help define their true nature. When these probes are combined with the usual safeguards of the scientific method--particularly the feedback and self-corrective elements in experimentation--then it can easily be seen that purposive behavior with all its teleological implications is a most

88

useful way for solving problems. In fact, without the inductive leaps and far vision that characterize purposive behavior, it is likely that man would be more apt than not to traverse static circles of knowledge or end up in cul-de-sacs during intellectual explorations. Purposive thinking, therefore, provides the foundation stone for developing a structure of conceptual representations for analyzing systems.

The point of view developed above is, of course, not unique. Many authors who have concerned themselves with problems of thinking and problem-solving have expressed similar views. Polya[*] in his preface to Volume I of Mathematics and Plausible Reasoning states that "all our knowledge outside mathematics and demonstrative logic . . . consists of conjecture." He goes on to say, ". . . we support our conjectures by plausible reasoning. A mathematical proof is demonstrative reasoning, but the inductive evidence of the physicist, the circumstantial evidence of the lawyer, the documentary evidence of the historian, and the statistical evidence of the economist belong to plausible reasoning. Demonstrative reasoning is safe, beyond controversy, and final. Plausible reasoning is hazardous, controversial, and provisional. Demonstrative reasoning . . . is . . . incapable of yielding essentially new knowledge about the world around us. Anything new that we learn about the world involves plausible reasoning, which is the only kind of reasoning for which we care in everyday affairs. Demonstrative reasoning has rigid standards, codified and clarified by logic (formal or demonstrative logic). . . . The standards of plausible reasoning are fluid, and there is no theory of such reasoning that could be compared to demonstrative logic in clarity or would command comparable consensus." Polya points out later that demonstrative reasoning and plausible reasoning do not contradict each other. Rather, "on the contrary, they complement each other. In strict reasoning the principal thing is to distinguish a guess from a guess, a more reasonable guess from a less reasonable guess." Later he states, "observe that inductive reasoning is a particular case of plausible reasoning."

Bartlett,[**] also supports the notion that there is utility in teleological reasoning. He states in his book Thinking that "it is more common for the steps to be reached through the terminal point than for the terminal point to be reached through the steps." He restates this point

* G. Polya, Mathematics and Plausible Reasoning, Vol I: Induction and Analogy in Mathematics. Princeton University Press, Princeton, New Jersey, 1954.

** F. Bartlett, Thinking: An Experimental and Social Study. Basic Books, Inc., New York, 1958, p. 23.

more clearly later on when he says we often "make a direct leap from the evidence given to an accepted terminal point, and the missing steps are then constructed on the basis of the already accepted issue."

A formidable question arises once the value of purposive behavior in problem-solving situations is seen. Where do the initial insights-- the inductive leaps--come from, and upon what do they depend?

Usually insights that make purposive behavior possible are accepted axiomatically. Frequently, solutions to problems are "seen" before they are proved. Einstein conceived his special and general theories of relativity before there was proof of them. So, too, the systems analyst undoubtedly conceives of useful ways of analyzing or conceptually representing systems before there is any direct evidence of the utility of these schemes. To understand the processes underlying insightful problem-solving behavior and thinking in general, one's first inclination is to turn to the pertinent technical literature. The available literature, however, is woefully inadequate for providing the understanding required.

Hebb* feels the problem of thought refers to "some sort of process that is not fully controlled by environmental stimulation and yet cooperates closely with that stimulation." He goes on to say that, "the failure of psychology to handle thought adequately has been the essential weakness of modern psychological theory." Humphrey** is of the opinion that, "fifty years experiment on the psychology of thinking or reasoning have not brought us very far . . .," and Bartlett*** has made the observation that none of the understanding of the psychology of skill (and thinking may be considered a form of higher-level skill) started from a formal analysis of laboratory situations. The literature is inadequate partly because strides reported deal primarily with narrow, simplified, and isolated problems that do not permit extrapolation to complex, real world problems. Thus, for example, a recent review of research on human problem-solving by Duncan† deals neatly and only with discrete, dependent, and independent variables that fall into three classes and that can be manipulated readily in the laboratory. One class of variables considered is transfer of problem-solving ability following variations in training.

* D. O. Hebb, The Organization of Behavior: A Neuro-physiological Theory, John Wiley & Sons, New York, 1949.

** Humphrey, op. cit., p. 308.

*** Bartlett, op. cit., p. 14.

† C. P. Duncan, "Recent Research on Human Problem-Solving," Psychological Bulletin, 1959, Vol. 56, pp. 394-429.

The emphasis typically is on such things as comparing the effect of memorization-type learning with "understanding"-type learning. A second class of variables pertains to changes in either the conditions under which problems are presented or changes in the problem itself. For example, relatively simple problems are presented in either concrete or symbolic forms. In other studies, the effects of hints and aids on problem-solving behavior were studied. A third class of variables pertains to differences between subjects such as sex, age, reasoning ability, and motivation.

Most of the literature on problem-solving, except the Gestalt literature,[*] describes the so-called stimulus-response approach. Here we usually deal with discrete, easily measurable stimulus materials and response reactions that fall into "right" and "wrong" categories. Many theoretical discussions, for example, Spence's,[**] focus on such phenomena as insight versus trial-and-error problem-solving, and continuity versus non-continuity in discrimination learning. Others have tried to gain understanding of the thinking process by simulating problem-solving behavior on a computer.[***] Newell and Simon record verbatim how a subject solves a problem that involves "recording" symbolic impressions. They postulate "that the subject's behavior is governed by a program organized from a set of elementary information processes." A set of subprograms are encoded for a digital computer; each subprogram "executes a process corresponding to one of these postulated information processes." The authors then state that they "write a program, compounded from these [subprograms] that will cause the computer to behave in the same way that the subject behaves--to emit substantially the same stream of symbols-- when both are given the same problem." The authors assume that if they "succeed in devising a program that simulates the subject's behavior rather closely over a significant range of problem-solving situations, then [they] can regard the program as a theory of the behavior." They look upon both the computer and the human as symbol manipulatory devices. They apparently can get their computer to go through some of the logic of symbol manipulations involved in verbalized thought processes

   * See for example M. Wertheimer, Productive Thinking.
     Ed. M. Wertheimer, Harper and Brothers, New York, 1959; J. P. Van
     de Geer, A Psychological Study of Problem-Solving, Uitgovery De
     Toorts, Haarlem, 1957.
  ** K. W. Spence, Theoretical Interpretations of Learning, Chapter 18;
     S. S. Stevens, Handbook of Experimental Psychology, Ed. Wiley, New
     York, 1951.
 *** A. Newell and H. A. Simon, "Computer Simulation of Human Thinking,"
     Science, 1961, Vol. 134, pp. 2011-2017.

associated with rather narrow and limited problems. But it is another matter indeed to create problem-solving thought processes in machines to deal with complex problems imbedded in highly unstructured situations representing high degrees of uncertainty. Since these are the situations in which the knowledgeable analyst must function, it is, to say the least, premature to expect to gain understanding of how these thought processes function by looking at computer thought simulation studies in their present stage of development.

Another theory on thinking is presented by Mark.[*] In his paper on thinking he is concerned "with phenomena which are likely to increase efficiency and versatility in problem-solving . . ." He believes "intelligent behavior in man is commonly associated with efficiency and versatility in information retrieval and learning as well as with behavior which generates new information which serves to answer old questions and create new ones." He attributes man's versatility in problem-solving to a memory "which can activate, maintain, and terminate activities independent of, or only indirectly related to, environmental or physiologic regulatory factors." He also attributes this versatility to "within-brain feedback controls" which provide "non-rigid probabilistic motivators." It is not clear what the author means by this, but, assuming these probabilistic motivators actually exist, they are supposed to allow flexible and logical operations of substitution. Mark states that because of these motivators "organisms . . . can learn to classify internal as well as incoming patterns as similar or different. This ability is considered to be a powerful tool which (i) permits a system to recognize the structure of the environment and (ii) further increase the system's operational capacities, ultimately leading from classification to statistical operations of predictions and an operation of measurement."

It is impossible on the basis of the foregoing articles to gain an understanding of the processes involved in purposive problem-solving that typifies the systems analyst's attempt to deal with systems analysis problems. Yet this is typical of the state-of-the-art. The attempt is mainly that of devising conceptual representations of relatively manageable and well-structured situations. There is little effort to deal with the relatively unmanageable and unstructured situation with which the systems analyst is frequently confronted.

The thinking processes involved in purposive behavior--a behavior which occurs commonly and is perhaps even necessary for effective

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* H. J. Mark, "Elementary Thinking and the Classification of Behavior," Science, 1962, Vol. 135, pp. 75-87.

problem-solving--cannot be clearly specified in the light of available knowledge. One can merely state but not adequately evaluate the different notions about thinking that have been expressed. For example, Bartlett[*] defines thinking as "the extension of evidence in accord with that evidence so as to fill up gaps in the evidence: and this is done by moving through a succession of interconnected steps which may be stated . . .or left till later to be stated." Bruner[**] et al, in A Study of Thinking, deal primarily with the cognitive process. By a cognitive process, the authors refer to "the means whereby organisms achieve, retain, and transform information." It must be assumed that this is also what they mean by thinking. They focus upon "the most ubiquitous phenomenon of cognition: categorizing or conceptualizing." They attempt to describe and measure "what happens when an intelligent human being seeks to sort the environment into significant classes of events so that he may end by treating discriminably different things as equivalents." Humphrey[***] states that thinking "may be provisionally defined as what occurs in experience when an organism, human or animal, meets, recognizes, and solves a problem. It is thus part of the total process of organic interaction with the environment." This latter statement, it will be recalled, is handled somewhat more carefully but perhaps even more vaguely by Hebb. In addition, as Humphrey points out, the situation is even more complicated by the consideration that "the psychology of thinking must deal not only with conscious but also with unconscious processes and motivation."

The above definitions of thinking are all very general. Most authors that pursue the topic quickly oversimplify the problem and reduce it to laboratory proportions. It might be more meaningful if the problem is to be pursued profitably to go back to the fundamental task of asking the appropriate question. As Bellman[†] has stated, "one cannot help feeling that too much effort has been devoted to obtaining answers without nearly enough effort being directed toward formulation of the proper question."

There is no doubt that one of the questions that needs to be asked is what constitutes good or adequate complex problem-solving behavior? Examples are needed to help define common areas of agreement. Other questions that must be asked include: How can we improve thinking?

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 * F. Bartlett, op. cit., p. 75.
 ** J. S. Bruner, J. J. Goodnow, G. A. Austen, A Study of Thinking. New York, John Wiley & Sons, Inc., 1956.
 *** G. Humphrey, op. cit., p. 311.
 † R. Bellman, Dynamic Programming, Intelligent Machines, and Self-Organizing Systems, RAND Corporation Memorandum RM-3173 PR, June 1962.

What are the important aspects of this process? What are the conditions, personality factors, and attitudes that positively or negatively affect this process? The characteristics of this behavior need to be classified and described in detail. But, because any approach to complex problem-solving behavior must be able to deal with relatively unstructured situations, with uncertain outcomes, with more than one adequate solution, and with more than one way to reach an adequate solution, even insight into the answers to some of these questions is likely to be insufficient to enable us to understand the thinking and problem-solving process much better than we do now.

Although this effort at understanding must be made, it may not necessarily lead to ways for enhancing the thinking process to any great degree. To make substantial gains in enhancing this process, it would seem more reasonable to return to the principle of having inductive hypotheses followed by deductive hypotheses and having these followed by test and verification of the hypotheses. The knowledge obtained then must be assimilated into and become part of the intellectual mesh--the unique historical accident--that for each analyst represents the sum and integration of all his experience. Thi new totality is applied to each new problem of finding a problem, each new effort of perceiving and structuring a problem, and each new teleological attempt to provide a direction and goal for problem-solving activities.

This, of course, is the iterative feedback method so common to the basic and applied sciences and disciplines. There is every good reason to believe that thinking, planning, problem-solving, and decision-making activities grow and benefit by use of this method. Thinking, planning, and so on do not become improved by mystical internal developments that are devoid of contact with and experience with the real world. Part of our thinking apparatus is the world around us, our externalized memory in the form of libraries and, above all, the tests we put to the world to answer various questions. So we may say that, if it is enhancement of thinking that we want, then this will come about in large measure through the extent and depth of the testing experience that each individual has the opportunity to gain. It must be this way. For if it is agreed that the uncertainty in complex problems is large, that there may be more than one right way to structure a problem, that there may be more than one right way to answer a problem and these answers cannot be known with certainty in advance, and that there may be more than one right path to reach many of the right answers that are possible, then, lacking omniscience, there is no alternative to developing an enhanced thinking capability other than through testing and retesting continually the world about us and the systems in it.

94

Given a basic level of intelligence, a basic level of motivation, a basic education, and a basic open-mindedness about the interpretation of events and facts as they appear, it would appear that it is the cumulative experience through test and retest that contributes in a very large way to the thinking process. This leads to the very simple, almost naive, conclusion that the best and most knowledgeable systems analysts are those with the most test experience.

VII   A BEHAVIORISTIC EXAMINATION OF THE KNOWLEDGEABLE ANALYST
by John B. Fink

Editor's Note: In the preceding chapter Rappaport
showed that the psychological factors which influ-
ence the knowledgeable analyst are extremely complex,
and thus do not lend themselves to a definitive ex-
planation. Fink in the present chapter shows that
a systematic description of the judgmental function
which the knowledgeable analyst performs is possible.
Fink demonstrates this by describing the operations
which a knowledgeable analyst must perform through
a stimulus-response discrimination model. On the
basis of this description Fink is then able to state
procedures for systems analysis.

What is a knowledgeable analyst?

This paper develops a concept of the "knowledgeable analyst" as an
operationally determinable set of behavioral events in specified environ-
mental contexts, and presents an operational procedure for systems
analysis.

In order to attribute operational, behavioral meaning to the concept
"knowledgeable analyst," we must provide operational, behavioral meaning
for the two components of the term. We must specify, as precisely as we
can, what we mean by analyst, and what we many by the qualifying condition,
knowledgeable.

To approach this operational, behavioral meaning, we begin by using
a process of empirically anchored induction to arrive at a tentative,
approximate definition of what the term under consideration must mean
empirically, i.e., the ways in which it is used, the situations in which
it occurs, the observable instances to which it refers. We scan these
ways, situations, and instances in order to abstract whatever character-
istics they may have in common. This communality provides the empirical
core of the term's definition. Where this communality contains terms
demanding further operational definition, we carry this out. We then
proceed to examine the available operational concepts with reference to
currently understood stimulus-response behavior theory.

97

Analysis

Analysis usually refers to such activities as observation, explanation, evaluation, problem-solving, and prediction of the phenomena to which the "analyst" addresses himself. The naming of these referential activities does not, in itself, provide operational meaning for analysis. Observation, explanation, evaluation, problem-solving, and prediction must, themselves, be defined operationally before analysis can take on operational meaning.

An analyst is, first of all, a behaver, i.e., he engages in a variety of specifiable activities that we call, collectively, behavior. Behavior consists of various behavioral events that can be identified as behavior patterns composed of smaller, identifiable behavioral events that we shall call behavior items. Thus, relatively minute behavior items, extended behavioral sequences, or complex simultaneous behavioral interactions may all be treated as behavioral events following, in general, the same set of principles of development and modification that provide prediction capability and enable control operations.

Observation

Observation requires a behavioral relation between some event available to be "observed" or responded to, and some organism that "observes" or responds to the event. An observer is a responder. Observation is respondent behavior.

Detailed examination of many empirical instances reveals that not only is observation to be defined as respondent behavior but conversely, every instance of respondent behavior may be correctly considered as an instance of observation. A dog's pricking up of its ears, a bird's turning of its head toward a specific sound, a man's "hello there!", or a systems analyst's identification of a system detail are all instances of observation behavior. Operationally, what they have in common is that each is an instance of a discrimination behavior; and this is true of all respondent behavior.

To discuss this a little more precisely, let us take a closer look at the behavioral activity of discrimination.

Discrimination may be defined initially as a selective, differentiated association of events available to be responded to and events that do the responding. For example, if an organism is surrounded by a set of environmental events, call them E events: E_1, E_2, E_3, . . . E_n, and if the

98

organism has at his disposal a set of behavioral events: B_1, B_2, B_3, . . . B_m, and if a subset of the available B events occurs with a high degree of reliability to a subset of the E events, we say that we are witnessing an instance of discrimination.

In Figure 1, $E_1 \longrightarrow B_2$ and $E_3 \longrightarrow B_1$ are instances of discrimination. When E_5 elicits B_4, B_5, B_6, and when E_7, E_8, and E_9 elicit B_8, we also have discrimination situations.

FIGURE 1

EXAMPLES OF ENVIRONMENTAL-
BEHAVIORAL DISCRIMINATION SITUATIONS
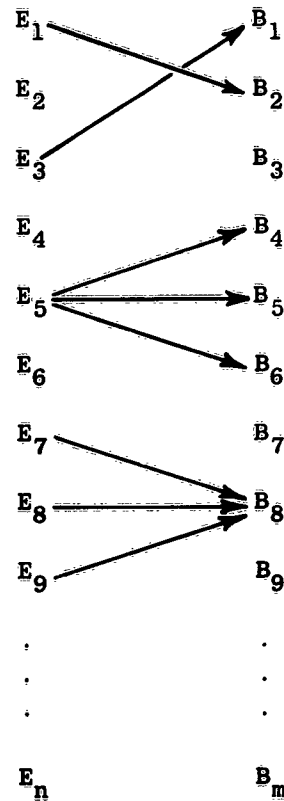
The smaller the size of the subsets and the higher the mutual-occurrence probability, the more effective the discrimination. The hypothetically limiting case of a single, narrowly differentiated B event related to a single, narrowly differentiated E event with a mutual-occurrence probability of 1.0 constitutes a theoretical ideal of optimum discrimination.

When the relation between a specified behavioral event, B_y, and a specified environmental event, E_x, assumes a probability discernibly distinct from chance or random contiguity, we are approaching the definitions of response and stimulus. The one other condition required is that of temporal relation.

Definition: When (1) the initiation of a specified environmental event, E_x, precedes the initiation of a specified behavioral event, B_y, and (2) the correlated occurrence of E_x and B_y assumes probability discernibly different from chance contiguity, then we call E_x a stimulus, S_x, and we call B_y a response, R_y.

The conditions that define stimulus and response are also the conditions that define discrimination. This is another way of saying not only that every S ⟶ R relation is a discrimination relation, but that every S ⟶ R relation is a respondent relation and an observation relation, i.e., the various expressions (1) S ⟶ R, (2) discrimination relation, (3) respondent relation, and (4) observation relation are operationally synonymous.

Schematically, as in Figure 2, E_1 ⟶ B_2 and E_3 ⟶ B_1 meet the conditions of a respondent, discrimination relation.

FIGURE 2

STIMULUS-RESPONSE REFLEXES
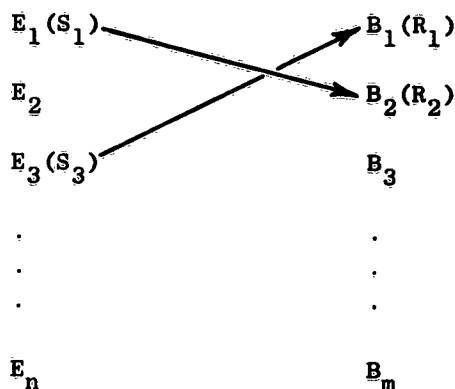
Therefore, we can refer to E_1 ⟶ B_2 and E_3 ⟶ B_1 as the stimulus-response relations, S_1 ⟶ R_2 and S_3 ⟶ R_1. A stimulus-response relation defined in this way is called a reflex.

100

In addition to referring to the simple general case of $S_x \longrightarrow R_y$ as a discrimination reflex, we further specify two types of situations: (1) stimulus discrimination, and (2) response discrimination.

Stimulus discrimination is the situation in which, as schematically represented in Figure 3,

FIGURE 3

STIMULUS DISCRIMINATION
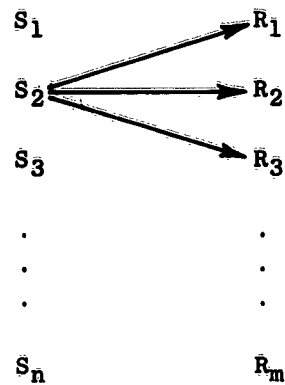
S_2 is selectively differentiated from its accompanying stimulus events S_1 and S_3 by the discriminative reflexes $S_2 \longrightarrow R_1$, $S_2 \longrightarrow R_2$, $S_2 \longrightarrow R_3$, whereas the situationally potential reflexes involving S_1 and S_3 do not occur.

Response discrimination is the situation in which, as schematically represented in Figure 4,

FIGURE 4

RESPONSE DISCRIMINATION
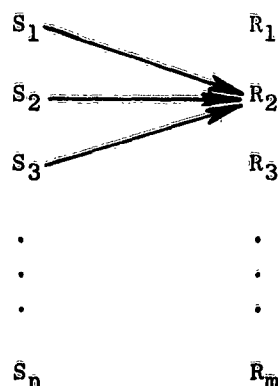
R_2 is selectively differentiated from available, alternative responses R_1 and R_3 by the discriminative reflexes $S_1 \longrightarrow R_2$, $S_2 \longrightarrow R_2$, and $S_3 \longrightarrow R_2$, whereas the situationally potential reflexes involving R_1 and R_3 do not occur.

Operationally, observation is discriminative reflex behavior. Conversely, every instance of reflex behavior, i.e., where an organism responds differentially to some environmental event, is an instance of observation behavior along with whatever other behavioral class to which its operational characteristics may assign it.

Explanation

Explanation, in its simplest form, is behavior that refers the occurrence or activity of some identifiable event to the occurrence or activity of another identifiable event. More complex forms of explanation are compounded of elements of this type. This definition not only covers, comprehensively, empirical instances of explanation, but also enables us to schematize explanation in fundamental stimulus-response terms.

Consider the general class of questions of the form: Under what circumstances does event $\underline{x}$ lead to event $\underline{y}$? i.e., what are the determining conditions for $E_x \longrightarrow E_y$?

Representing the operational elements of this question schematically (Figure 5), we are asking: When E_x and E_y appear in a larger environmental

102

context also containing E_1, E_2, E_3, . . . E_n, which of the E conditions determine the occurrence of E_y subsequent to the occurrence of E_x?

FIGURE 5

SCHEMATIC REPRESENTATION
OF THE "PRE-EXPLANATION" SITUATION

E_1

E_2

E_3

.

. E_y

.

E_x

.

.

.

E_n
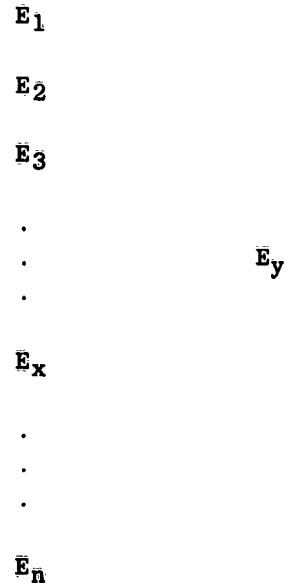
Since "explanation" operationally implies an observer-explainer who responds to these events, the E events are actually S events. We are really asking: What are the S_n conditions that determine the occurrence of S_y subsequent to S_x? Or, what other S events must accompany S_x to determine the occurrence of S_y?

A stimulus-response schematic (Figure 6) shows the observer-explainer responding to all identifiable events in the situation, and to their correlated occurrences.

FIGURE 6

SCHEMATIC REPRESENTATION OF "EXPLANATION"

$$S_1 \dashrightarrow R_1$$

$$S_2 \qquad\qquad R_2$$

$$S_3 \dashrightarrow R_3$$

$$S_4 \dashrightarrow R_4$$

$$S_5$$

$$\begin{aligned} & R_1 \\ & R_3 \\ \bullet \quad S_y \dashrightarrow & R_y \\ \bullet \quad\quad & R_x \end{aligned}$$

$$S_x$$

$$\vdots$$

$$S_n$$
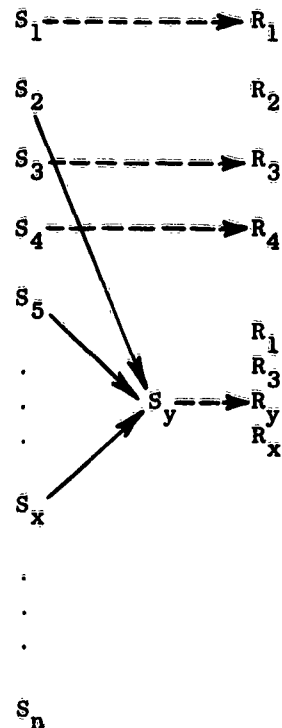
In this example, the observer responds to S_1, S_2, S_3, S_4, S_5, . . . S_x, . . . S_n, and S_y when they happen to occur. The observer also responds to the highly correlated occurrences of $S_2 S_5 S_x \longrightarrow S_y$. S_1, S_3, S_4 are not observed to accompany S_x in highly correlated occurrences with S_y. Therefore, S_2 and S_5 are specified as co-determining explanatory stimuli for the relation $S_x \longrightarrow S_y$; and S_1, S_3, and S_4 are not.

A generalized definition statement of "explanation" consists of three conditions and a conclusion:

...When the occurrence of a specified event y follows the occurrence of a specified antecedent event x; i.e., $x \longrightarrow y$,

...when the correlated occurrence of x and y assumes a probability value discernibly different from chance contiguity, and

...when the occurrence of other specified events v and w is corre-
lated beyond chance contiguity with the mutual occurrence of x
and y,

...then v and w are said to be explanatory co-determinants of the
relation $x \longrightarrow y$.

Note the similarity between this definition of explanation and the
definition of a stimulus-response reflex on page 100. The form and sub-
stance are essentially the same except for the presence of additional
events v and w associated with $x \longrightarrow y$ in the present discussion. This
suggests that we can say that in any stimulus-response reflex situation,
S not only elicits R but constitutes an operational explanation for the
occurrence of R; and any other events significantly correlated with S as
it engages in its $S \longrightarrow R$ relation are explanatory events for the S-R
relation, and co-determining explanatory events for the occurrence of R.

If v and w correlate with $x \longrightarrow y$ at significantly different proba-
bility levels, the higher probability level is assigned the explanatory
function.

Evaluation

Evaluation, via operational induction, turns out to be a behavioral
comparison process, a comparison of an available set of events currently
under investigation, with a set of events defined to be the objective.
This comparison is made in terms of observable stimulus characteristics
by the "evaluator," again our observer. In its simplest form, evaluation
might be represented schematically, as in Table 1.

105

Table 1

EVALUATION TABLE

Objective Stimulus Set	Available Stimulus Set	Evaluation Operations	
		Specification of Correspondences	Specification of Discrepancies
S_1			S_1
S_2	S_2	S_2	
S_3	S_3	S_3	
S_4	S_4	S_4	
S_5	S_5	S_5	
S_6			S_6
S_7	S_7	S_7	
	S_8		S_8

The evaluator observes that the available stimulus set corresponds to the objective stimulus set with respect to stimuli S_2, S_3, S_4, S_5, and S_7. He observes discrepancies between available and objective stimulus sets with respect to S_1, S_6, and S_8. He notes further that this is due to the absence of S_1 and S_6, and the excessive presence of S_8 in the available stimulus set. This is the end of his role as evaluator. If he were to go on as a problem-solver, it would be his task to designate operations to add S_1 and S_6 to the available set, and to remove S_8 from it.

Problem-solving

From the point of view of the problem-solver, there are two operationally distinct kinds of problems: we shall label these (1) the vague-uneasiness problem, and (2) the stimulus-oriented problem.

The vague-uneasiness problem exists for the problem-solver when unspecified circumstances lead him to say: "I am aware that something is wrong, but I am unable to say precisely what it is or what it consists of." This kind of problem is resolved by a recognition and specification of the second kind of problem, the stimulus-oriented problem.

In some cases, resolution of the vague-uneasiness problem, by specifying the stimulus-oriented problem, solves the practical "problem" at hand; i.e., the problem-solver may say: "Now that I know what was bothering me, I am no longer bothered. I have no further problem." In other cases, the total problem-solving situation begins with the vague-uneasiness problem, and transitions into the stimulus-oriented problem which must be solved before the total "problem" is considered solved.

Effective problem-solving behavior involves a number of clearly specifiable, operational steps:

1. Specification of types of stimulus events that enter into the problem

2. Specification of stimulus characteristics and interactio s that are desired

3. Specification of stimulus characteristics and interactions that are currently observable

4. Specification of discrepancies between (3) and (2)

5. Specification of operations converting (3) to (2)

6. Testing validity of operations specified for converting (3) to (2)

7. Conducting operations validated for converting (3) to (2)

Steps 1 through 4 define the stimulus-oriented problem. Their relation to our operational definition of evaluation (page 106) is obvious. Steps 5 and 6 provide analytical and methodological solutions for the problem. Step 7 provides empirical solutions for particular in situ instances of the general problem.

107

Decision-making

The systems analyst is faced repeatedly with situations that require decisions. Given a specified set of mission requirements, which of a number of alternative performance functions should he incorporate into his system? Which of a number of available, structural components should he select to achieve the desired functions? Faced with a cost-time trade-off in research and development planning, which of a number of available cost-time combinations should he utilize as optimum? Decision situations confront him everywhere. How does the analyst "arrive" at the necessary decisions? What is decision-making? What are its determining factors?

In stimulus-response terms, decision-making consists of performing, discriminatively, one of a number of available responses; i.e., decision behavior is an instance of response-discrimination as schematically represented in Figure 4. There is nothing behaviorally unique about what we are calling decision-making. However, the determining factors in decision behavior are worth some discussion.

What are the determining factors in decision behavior? To phrase this in our technical behavior language: What are the determinant variables in response-discrimination?

Initially we can say that there are two general types of response-determining variables: (1) immediate stimuli, and (2) reflex history. The importance of the Type 1 variable, consisting of immediate stimuli, is self-evident. That a person responds to whatever is present is beyond argument. The significance of the Type 2 variable, reflex history, may require some explanation.

While it is true that a person responds to whatever stimuli are present, their presence alone does not determine the specific nature of the response. Referring again to Figures 3 and 4, the presence of any one or several of the stimuli S_1, S_2, S_3, . . . S_n will, of course, elicit the occurrence of one or more of the responses R_1, R_2, R_3, . . . R_m. But the particular response or response combination that actually does occur is determined by the reflex history of the person with respect to these stimuli and responses.

If a stimulus, say S_2, that is present on some specific occasion is involved in unconditioned (i.e., unlearned or "instinctive") reflex with one specific response, say R_2, then $S_2 \longrightarrow R_2$ has an effective reflex strength. S_2 determines the occurrence of R_2. And we can say that R_2 is the behavioral "decision" consequence of S_2.

Some critics might argue that the term "decision" does not ordinarily apply to unconditioned, "instinctive" reflex behaviors, that it usually refers to situations where the person is not compelled by biological necessity to provide a specific response. This is a false argument; i.e., it implies a premise that, in careful experimental observation, is empirically and patently false. It implies that non-"instinctive" reflexes, i.e., conditioned (learned or "acquired") reflexes, are not instances of biological necessity; that, somehow, there is some element of subjective "choice" (the traditional ethical philosopher's predilection for "free will"). Unfortunately, a reluctant but inevitable statement derived for rigorous behavior research and behavior theory is that acquired behavior is just as thoroughly determined as is unconditioned, "instinctive" behavior.

Briefly, let us take a closer look at the apparent paradox of choice. We find ourselves confronted by a situation that, environmentally, allows us to make any one of a number of responses. This is a choice situation defined in terms of what the physical environment allows. We "feel" that we have a choice. However, our reflex conditioning history predisposes us to make R_1, R_2, or R_3; or $R_1 + R_2$; or it produces conflicting behaviors R_1 and R_3 so that, as a result of neuromuscular competition, we are either unable to make R_1 or R_2 (i.e., they are mutually inhibiting), or we oscillate between R_1 and R_2. It is this last situation that gives us the illusion of choice. Since it isn't very flattering to accept this mechanistic, deterministic view of our behavior, we generally continue to beguile ourselves with this illusion of free choice.

We are saying that experimental evidence on behavior suggests that stimulus-defined choice situations do exist, but that response-defined choice situations do not.

The effect of this on our decision-maker is clear. He can be presented with choice-problems, but he emerges with a determined, non-choice response solution.

Hesitant, oscillatory behavior is often interpreted as "choice behavior." This is illusory. Hesitant, oscillatory response patterns are as thoroughly determined as any other responses; and if they are subsequently followed by some unambiguous "decision" response, then it, too, is determined. It has finally been evoked by either some tardily observed stimulus or by the occurrence of some available, previously well-established response-response reflex (i.e., $R_1 \longrightarrow R_2$).

What this means to our "decision-making" analyst is that (1) his reflex history determines how he will respond to specified stimuli,

(2) available, immediate stimuli in conjunction with that reflex history determine how he will respond now.

Decision-making is discriminative-response behavior--no more, no less. Since we can do nothing to change this state of affairs, we can at least note one advantage in it. It enables the deterministic training of decision behavior.

Prediction

In view of the discussion to this point, prediction becomes a relatively simple case of extimating $S_x \longrightarrow S_y$ probabilities based upon the reflex probability history of the observer.

Operationally, to what kinds of situations does the term prediction refer?

Let us say that we wish to predict some event, y, with respect to some event, x. This requires (1) that we so define the characteristics of x that we can identify its occurrence or non-occurrence; (2) that x belongs to a group of x s that display a set of similar, identifiable characteristics that we define as the characteristics of the group of x s; we refer to this group of x s as the class of x.

Having defined the class of x in terms of a certain limited number of commonly displayed, identifiable characteristics, we now proceed to examine a sample of these x s to see what other characteristics they may possess; let us call these x' ("x-prime") characteristics.
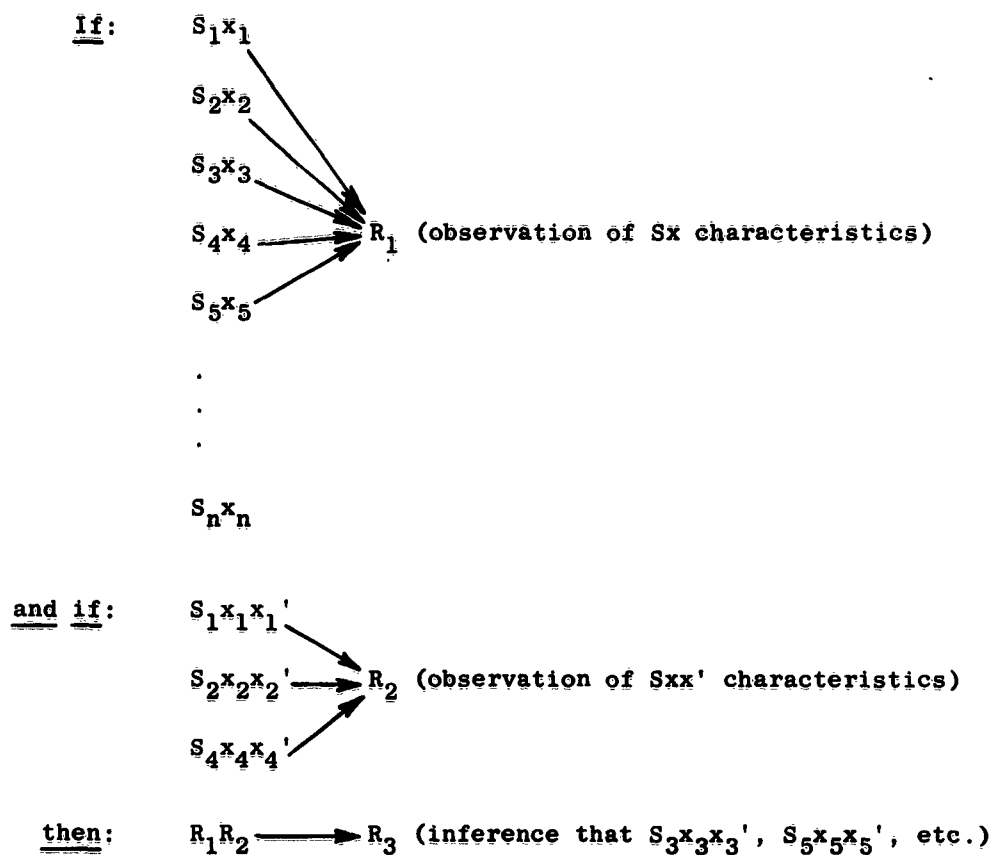
We now say, to the extent that the events we call x s are validly x s (i.e., to the extent that they actually display the originally defined x characteristics), and to the extent that these x events actually display x' characteristics, then to that extent we will expect other members of the x class not yet examined also to display x' characteristics. Thus, we predict the occurrence of x'. If some event y fits the definition of x', then y is an x'; and we can predict y.

Prediction, as defined here, is a legitimate method of extrapolation from a sample to the class, where the class has been defined validly (i.e., where the characteristics of the member events have been defined as identifiable and reliable occurrences for all members), and where the sample has been selected validly (i.e., where all members of the sample conform to the defining characteristics of the class).

110

Since, operationally, the $\underline{x}$ and $\underline{x}'$ characteristics are stimuli for responses of an observer, our prediction situation may be schematized in S-R terms, as in Figure 7. $\underline{\text{If}}$ S_1, S_2, S_3, S_4, S_5, . . . S_n are observed to manifest characteristics x_1, x_2, x_3, x_4, x_5, . . . x_n; and, $\underline{\text{if}}$ $S_1 x_1$, $S_2 x_2$, and $S_4 x_4$ are observed to manifest characteristics x_1', x_2', and x_4', respectively; $\underline{\text{then}}$ it is inferred that $S_3 x_3$ will manifest x_3', $S_5 x_5$ will manifest x_5', etc.

FIGURE 7

SCHEMATIC REPRESENTATION OF "PREDICTION"

$\underline{\text{If}}$: $S_1 x_1$

$S_2 x_2$

$S_3 x_3$

$S_4 x_4 \longrightarrow R_1$ (observation of Sx characteristics)

$S_5 x_5$

.
.
.

$S_n x_n$

$\underline{\text{and if}}$: $S_1 x_1 x_1'$

$S_2 x_2 x_2' \longrightarrow R_2$ (observation of Sxx' characteristics)

$S_4 x_4 x_4'$

$\underline{\text{then}}$: $R_1 R_2 \longrightarrow R_3$ (inference that $S_3 x_3 x_3'$, $S_5 x_5 x_5'$, etc.)

If we have a number of stimuli, S_1, S_2, S_3, etc., with $\underline{x}$ properties, and if a sample of these Sx events show $\underline{x}'$ properties, then we infer that other Sx events show $\underline{x}'$ properties.

Prediction is the asserted extension of x' characteristics from x-class sample members for which x' observations have been made to x-class members for which x' observations have not yet been made.

The Meaning of "Knowledgeable"

What do we mean by "knowledgeable?" Operationally, "knowledgeable" must refer to some identifiable behavior pattern, some set of observable responses.

When we ask whether George "knows" that event E_1 leads to event E_2, we are asking, operationally, whether when presented with E_1, either directly or symbolically (e.g., verbally), George will behave as if he expects E_2 to follow. This behavior can take the form of a symbolic response (e.g., verbal) or a direct response indicating expectation or prediction of E_2.

The definition of "knowledgeable" must meet two conditions: (1) situational validity, and (2) reflex reliability.

Situational validity requires an external criterion; i.e., it is concerned with whether George's response corresponds to some other criterion for determining which of a number of possible responses is the "correct" response. For example, if in the year 1400 a school child were asked, "What is the shape of the earth?", his "correct" response, demonstrating his "knowledge," would have been "Flat!" This would have been "correct" and "knowledgeable" in the sense that it was coherent with the general opinion of the time. In 1900, the "correct" and "knowledgeable" response with respect to general opinion would have been "Round!" In 1962, with reference to various scientific findings, a more "knowledgeable" response would be "Roughly pear-shaped!"

In stimulus-response terms (Figure 8), of three possible responses to S_x, namely $R(S_a)$, $R(S_y)$, and $R(S_b)$,

112

FIGURE 8

SCHEMATIC REPRESENTATION OF "SITUATIONAL VALIDITY"

$$R(S_a)$$

$$S_x \xrightarrow{\hspace{1.5cm}} R(S_y) \xrightarrow{\hspace{1.5cm}} S_v$$

$$R(S_b)$$
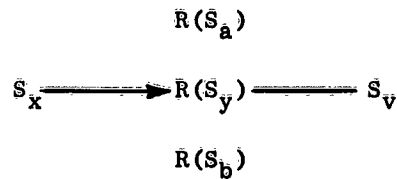
response $R(S_y)$ is the "correct" or "knowledgeable" response because it is accompanied by the validating criterion stimulus, S_v.

Reflex reliability refers to the situation where, to the repeated occurrence of stimulus S_x, response $R(S_y)$ has a high probability of subsequent occurrence. Reflex reliability is a necessary condition for determining the specification of "knowledge" because random occurrence alone would account for an occasional, situationally valid, "correct" response. But, if reflex reliability is discernible at an acceptable probability level, then we can say that the "correct" response is "knowledgeable" rather than random.

With reference to our earlier discussion, "knowledgeable" behavior is, obviously, discrimination behavior. Discrimination behavior always meets the reflex reliability condition, for this is what enables us to identify it as discrimination behavior. But, discrimination behavior, as such, does not always meet the situational validity condition. It is this condition that enables us to distinguish "knowledgeable" discrimination responses from "non-knowledgeable," "incorrect," or "false" discrimination responses.

The knowledgeable analyst, then, is an observer who engages in discrimination reflex behaviors that meet the conditions of situational validity and reflex reliability; and he employs these discriminative reflexes in the operational, stimulus-response activities of explanation, evaluation, problem-solving, and prediction.

A Behavioral Definition of Meaning

We are engaged in an operationally oriented investigation designed to arrive at a concept of the "knowledgeable analyst" as an operationally determinable set of behavioral events. To accomplish this, we have

inquired into the meaning of "analyst" and of the qualifying condition, "knowledgeable." But, we have not yet defined what we mean by meaning.

"What do we mean by meaning?" is itself not a legitimate question since the use of the word "mean" implies that we understand it while at the same time we are inquiring about "meaning." An operationally oriented rewording of the question resolves this problem by asking, "To what kinds of events does the term meaning refer?"

Beginning with a broad scan of empirical instances, we can assert that wherever a responder is available, meaning is a relation between some observed event and a responder. We, as outside observers or meta-observers, further observe that various responders often respond differently to what is defined by the meta-observer as the "same" event.

When the meta-observer says that observable instance x is the "same" event as observable instance y, he is saying that his response to y is the same as his response to x. And here is our clue to a referential definition of meaning. x "refers to" y for the meta-observer in the sense that his response to x, call it R_x, is no different from his response to y, R_y; i.e., when R_x and R_y are non-differentiable, then the meaning of x which is R_x, and the meaning of y which is R_y, are non-differentiable. When R_x is R_y, then x means y.

This holds not only for the meta-observer, who is a responder, but also for the responders that he is observing, and the events to which they are responding.

Meaning is responding. The meaning of a stimulus to a responder is precisely the response that he provides.

A nice example of this is available in the meanings of the visual stimulus-word "CHAT." If we ask English-speaking responders, "What is the meaning of 'CHAT'?", they respond in terms of such referents as "talking, conversing, discussing," etc. The same question to an audience of French-speaking responders, couched of course in French, "Qu'est-ce qu'il veut dire 'CHAT'?", brings responses in terms of a hairy animal with specifiable anatomical characteristics, that produces a sound something like "Meow." The referent for the French "chat" is the referent for the English "cat." So, what does "CHAT" mean? It means precisely the response that the observer provides to it.

A distinction is sometimes made between correspondent meaning and coherent meaning. Correspondent meaning is defined as an assigned correspondence between the event to be defined (the definiendum) and the

114

event doing the defining (the definiens). <u>Coherent</u> meaning says that an event is defined by its context, that the surrounding events structure the meaning of the event under consideration. There appears to be nothing wrong with these two "types" of meaning as long as we go on to note the observable fact that they achieve their "meaning" properties only by virtue of the behavior of a responder.

Thus if, as in "coherent" meaning, an observer responds to $\underline{x}$-stimuli in a context of $\underline{p}$-stimuli as "x_p," and to $\underline{x}$-stimuli in a context of $\underline{q}$-stimuli as "x_q," then it is his response to the context stimuli along with his response to the $\underline{x}$-stimuli that defines "x_p" and "x_q" respectively, and provides the distinction between them (Figure 9).

FIGURE 9

COHERENT MEANING
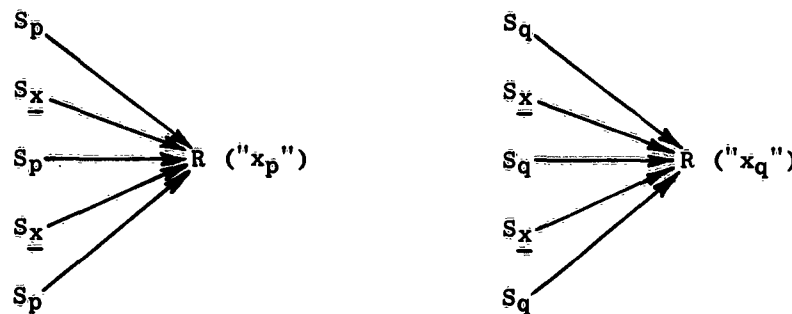
The schematic in Figure 9, representing coherent meaning, is actually an instance of stimulus discrimination. If the observer always saw $\underline{x}$ immersed in a context of randomly mingled $\underline{p}$'s and $\underline{q}$'s, he would make no distinction between x_p and x_q; there would be only $\underline{x}$ with random associative properties. It is only when he observes $S_x S_p$ and $S_x S_q$ combinations grouped homogeneously that he emerges with the stimulus discrimination, S_{xp} and S_{xq}.

Similarly for "correspondent" meaning, as in Figure 10.

FIGURE 10

CORRESPONDENT MEANING
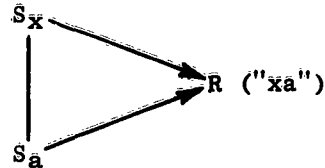
When an observer responds to S_x and S_a in correlated occurrence, then S_x is defined in association with S_a.

Both "coherent" meaning and "correspondent" meaning depend upon stimulus association in reflex relation to a discriminative response. The "two types" of meaning are subcategories of this general behavioral principle of discriminative response to stimulus association.

Behaviorally, the meaning of any stimulus is the response of the observer. Behaviorally, meaning is responding.

An Operational Procedure for Systems Analysis

The behavioristic picture of the knowledgeable analyst developed here holds specific implications for the kind of procedures he will employ in his systems analysis activity. The behavioristically, operationally oriented analyst tries to provide explicit identification and definition for the events, concepts, and operations that he considers critical to the systems analysis task.

It would be pointless to assert that he will actually explicate every one of the critical items; this would be a non-testable assertion. The important point here is that he attempts to identify every critical item explicitly. His methodological assumption is that greater explicitness is more productive of over-all operational system reliability than less explicitness, even though total explicitness may not be achievable.

Thus, our definition of the knowledgeable analyst provides a basis for stating the functions involved in operational procedures for systems development, systems evaluation, and systems compatability.

116

In the systems development problem the behavioristically oriented analyst might phrase his task as: "We are interested in developing a system that has certain mission objectives with respect to certain environmental events and certain minimum performance requirements."

In performing this task, he will engage in the following types of operations:

1. Specification of environmental events E_1, E_2, E_3, . . . E_n, environmental objectives and constraints with respect to which the system is intended to operate; these are the environmental determinants of the system.

2. Specification of mission objectives M_1, M_2, M_3, . . . M_n, and examination of these to see if they are operationally meaningful in terms of the population of environmental events with which they are intended to deal; i.e., M_1, M_2, M_3, . . . M_n are validated against E_1, E_2, E_3, . . . E_n. Validation in this context means: Do the specified M events refer operationally to specified E events; and are the M events, as specified, necessary and sufficient to meet specified E event conditions?

3. Specification of minimum system performance requirements P_1, P_2, P_3, . . . P_n demanded by mission objectives M_1, M_2, M_3, . . . M_n. Validation of P parameters against M parameters to meet conditions of reference, necessity, and sufficiency. Examination of P parameters to see if they are operationally feasible; i.e., validation of P parameters against E parameters.

4. Specification of minimum component functions requirements F_1, F_2, F_3, . . . F_n demanded by performance requirements P_1, P_2, P_3, . . . P_n, and validation of F parameters against P parameters.

5. Specification of structural characteristics S_1, S_2, S_3, . . . S_n demanded by F parameters and P parameters. Validation of S parameters against F parameters and P parameters.

6. Specification of predicted performance characteristics PP_1, PP_2, PP_3, . . . PP_n determined by F and S parameters. Validation of these PP parameters against P parameters.

Step 1 specifies the environmental determinants--the original set of events with respect to which the system is to be developed. Steps 2 through 6 involve successive generation and validation of each subsequent set of events with respect to its antecedent set.

117

In an idealized situation, with no constraints, the analysis would be complete at this point. However, the practical, "knowledgeable" analyst is aware that in the real world of empirical, operational systems, there are some very real constraints. He knows that he must consider structural constraints (K_s), cost constraints (K_c), and time constraints (K_t). Consequently, he will employ further steps. He will:

7. Specify <u>structural constraints</u> K_{s1}, K_{s2}, K_{s3}, . . . K_{sn} imposed by the limiting characteristics of available component materials.

8. Specify <u>cost constraints</u> K_{c1}, K_{c2}, K_{c3}, . . . K_{cn} imposed by available funds, materials, processes, procedures, and manpower.

9. Specify <u>time constraints</u> K_{t1}, K_{t2}, K_{t3}, . . . K_{tn}; initially the over-all time limit for total system development; (later, time limits imposed upon various phases of the system development program).

10. Examine the <u>cost x time trade-off function</u>, and select an acceptable range of values. The curve in Figure 11 shows a simple, generalized cost x time trade-off function. Extra-system requirements define the maximum cost (max-cost) constraint and the maximum time (max-time) constraint. The max-cost and max-time values are projected to intersect the curve. That area of the curve bounded by the max-cost and max-time intersects contains the range of acceptable cost x time trade-off values.

11. Restate the structural constraints of Step 7 as affected by the acceptable range of cost x time trade-off values in Step 10, thereby providing revised structural constraints, K'_s.

12. Restate the structural characteristics, the S parameters of Step 5, as modified by the revised structural constraints, K'_s, of Step 11, thereby providing <u>revised structural characteristics</u>, S'.

13. Identify the effects of the revised structural characteristics, S', developed in Step 12, on functions requirements (F, step 4) to yield <u>revised functions requirements</u>, F'.

14. Identify the effects of S' characteristics and F' characteristics on predicted performance (PP, Step 6) to yield <u>revised predicted performance</u> characteristics, PP'.

FIGURE 11

COST x TIME TRADE-OFF FUNCTION
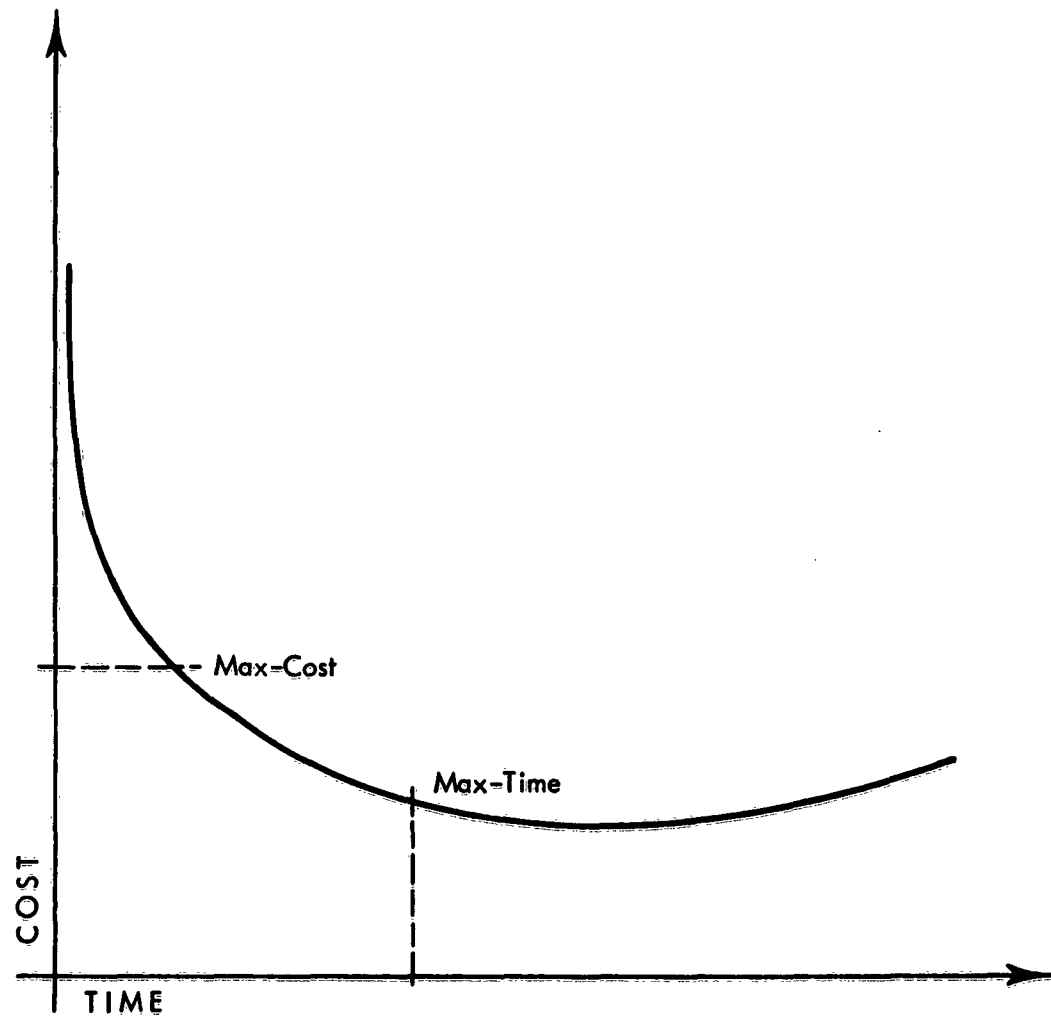
15. Compare PP' characteristics with original performance requirements (P, Step 3). Note discrepancies, and specify revised performance requirements, P'.

16. State revised mission objectives, M', to fit available PP' and P' characteristics.

In practice, the knowledgeable analyst recognizes that revised predicted performance, PP', is his critical dependent variable. It is the center of his activity. Its parameters are determined by certain positive functions that support PP characteristics, and by certain negative functions that depreciate PP characteristics.

The effective, knowledgeable systems analyst utilizes these 16 basic activities in phases, as indicated in Table 2.

In general, the activities of Phase I deal with positive, supporting functions. If we represent the validation procedures involving Steps 2 through 6 in functional terms, then: $M = f(E)$, $P = f(M)$, $F = f(P)$, $S = f(F)$, and $PP = f(S, F)$; i.e., mission objectives are a positive function of environmental determinants, performance requirements are a positive function of mission objectives, functional requirements are a positive function of performance requirements, structural requirements are a positive function of functional requirements, and predicted performance characteristics are a positive functional consequence of structural and functional characteristics in interaction.

A curve representing each of these functions would be of the general form indicated in Figure 12, where the dependent variable is an increasing, monotonic function of the independent variable.

FIGURE 12

GENERAL FORM OF THE FUNCTIONAL RELATIONS
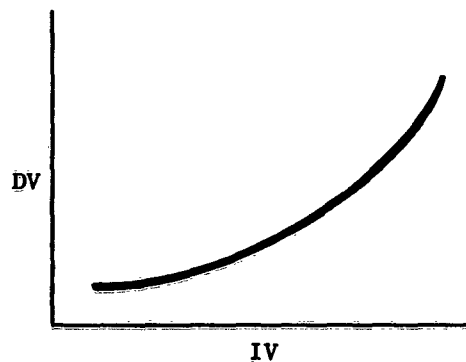OF DEPENDENT AND INDEPENDENT VARIABLES IN PHASE I

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120

Table 2

STEP AND PHASE BREAKDOWN OF SYSTEMS ANALYSIS PROCEDURE

Phase	Step	Relevant Events
I	1	E: environmental determinants
	2	M: mission objectives
	3	P: performance requirements
	4	F: functions requirements
	5	S: structural characteristics
	6	PP: predicted performance
II	7	K_s: structural constraints
	8	K_c: cost constraints
	9	K_t: time constraints
	10	K_c: cost constraints K_t: time constraints
	11	K_c: cost constraints K_t: time constraints K_s': revised structural constraints
	12	K_s': revised structural constraints S': revised structural characteristics
III	13	S': revised structural characteristics F': revised functions requirements
	14	S': revised structural characteristics F': revised functions requirements PP': revised predicted performance
	15	PP': revised predicted performance P': revised performance requirements
	16	P': revised performance requirements M': revised mission objectives

Steps 7 through 12, constituting Phase II, deal with constraining variables that have the effect of depreciating structural characteristics, S. As indicated in Step 7, limiting characteristics of available component materials impose structural constraints, K_s, upon structural characteristics, S; i.e., $S' = f(K_s)$, of the general form indicated in Figure 13.

FIGURE 13

EFFECTIVENESS OF REVISED STRUCTURAL CHARACTERISTICS
AS A FUNCTION OF EXTENT OF STRUCTURAL CONSTRAINTS
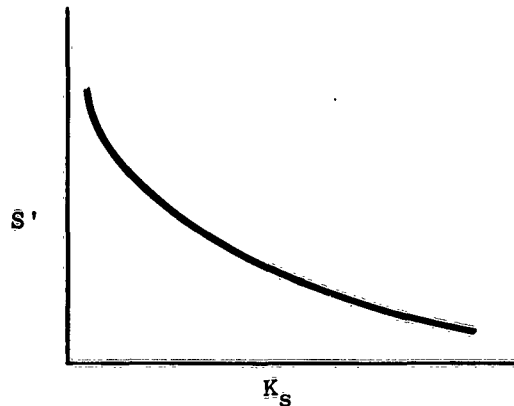
As structural constraints, K_s, increase, effectiveness of revised structural characteristics, S', decreases. Furthermore, for any specified complex system, structural constraints, K_s, are inversely related to money spent (i.e., cost) and to time spent; that is, within certain ranges of values, the less money and time spent on research and development, the greater the magnitude of structural constraints imposed on the structural effectiveness of the system. To express this entirely in constraint terms, the greater the size of cost and time constraints, the greater the structural constraints. K_s is a positive function of K_c and K_t (Figure 14) so that S', in turn, is a negative function of K_c and K_t (Figure 15) as effected through K_s (cf. Figure 13).

FIGURE 14

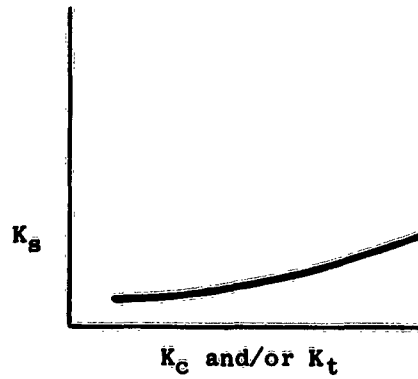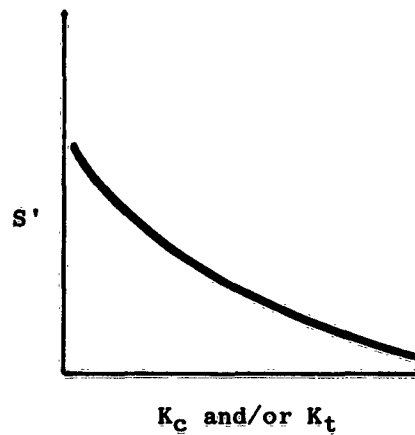STRUCTURAL CONSTRAINTS AS A
FUNCTION OF COST AND TIME CONSTRAINTS

K_s

K_c and/or K_t

FIGURE 15

STRUCTURAL EFFECTIVENESS AS A
FUNCTION OF COST AND TIME CONSTRAINTS

S'

K_c and/or K_t

Phase III, Steps 13 through 16, deals with the effects of these S'
determinants on functions requirements, F, to produce revised functions
requirements, F'; and consequentially on predicted performance, PP, to
produce revised predicted performance characteristics, PP'. PP', in turn,

123

can demand revised performance requirements P' and revised mission objectives, M'. Since PP' characteristics are a positive function of revised structural characteristics, S', anything that encourages structural constraints, K_s, has the effect of depreciating predicted performance. Wherever PP' characteristics differ from original system performance requirements, P, these P requirements must be revised as P' requirements to correspond to the operational limits of the PP' characteristics. If this revision demands a modification of the original mission objectives, then these M events must be revised to M' events compatible with PP' and P'. In other words, when a systems development problem is conducted rigorously by an honest, competent, knowledgeable analyst, we do not always emerge with precisely the originally intended mission. However, we do emerge with the nearest possible operational approximation. And, we do know precisely how and where we deviate, and why!

The systems development procedure we have outlined here is actually a successive validation technique. We validate M against E, P against M, F against P, S against F, PP against S and F, etc., right on through the process. We further validate S against the constraining functions K_s, K_c, and K_t to yield S', F against S' to yield F', PP against F' and S' to yield PP', P against PP' to yield P', and, finally, M against P' to yield M'. If M' = M, then the original mission definition is unchanged. It is a rigorous validation sequence: mission definition against environmental determinants, system characteristics against mission definition, system characteristics against externally imposed constraints yielding revised system characteristics, mission definition against revised system characteristics yielding, where necessary, a revised mission definition.

Now, this kind of validation is, basically, nothing more than the stimulus discrimination comparison we considered in our discussion on evaluation (p. 106). Beginning with the M against E validation, and continuing through P against M, F against P, etc., each successive validation comparison consists of an Objective Stimulus Set and an Available Stimulus Set (cf. Table 1). The major difference is that the Available Stimulus Set is generated from the Objective Stimulus Set before being compared (validated) against it.

There are a variety of ways in which this successive validation technique can be performed. Ideally, we should have some standardized, procedural form or check-list model with respect to which detailed validation relations can be indicated to and checked off by the systems analyst. One model of this kind is the Systems Analysis and Integration Model

(SAIM) developed originally by Shapero and Bates,[*] and discussed by Shapero and Schaeffer.[**] This model arranges for specification of critical systems-analytic events and is also nearly identical with the first two steps of the matrix-network approach presented by Schaeffer in Chapter III.

No discussion of the system analyst's task is complete without considering the problem of reliability. Reliability is an operationally definable concept that refers to a measure of correspondence or correlation between two or more specified sample sets of events drawn from the same event-population. When we investigate system performance reliability, we are inquiring as to how well, on a number of successive system performance trials, the operational performance (OP) characteristics correspond or correlate. That is, if Trial 1 operational performance consists of items OP_1, OP_2, OP_3, . . . OP_n, and Trial 2 performance consists of items OP'_1, OP'_2, etc., and Trial 3 consists of OP''_1, OP''_2, etc. then how well do the OP, OP', OP'' characteristics correlate? There is no need to discuss here the mathematical correlation model, since it is routinely available in any comprehensive statistics text. Let us observe, however, that this correlation comparison of repeated performance patterns is a specific example of the validation (evaluation), discriminative stimulus comparison discussed previously (p. 106).

System evaluation, which is a major problem discussed by Schaeffer in Chapters I and II, consists of comparing revised predicted performance characteristics, PP', with operational performance characteristics, OP; i.e., does the system do what it is supposed to do? This is actually system validation against operational performance.

System compatibility analysis compares PP' characteristics of an interacting subsystem, or OP characteristics of one subsystem with OP characteristics of an interacting subsystem. This is an inquiry into intersubsystem validity; i.e., how does each subsystem validate with respect to its interacting subsystems?

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[*]   Albert Shapero and Charles Bates, Jr., A Method for Performing Human Engineering Analysis of Weapon Systems, WADC Technical Report 59-784, Aerospace Medical Laboratory, Wright Air Development Center, Wright-Patterson Air Force Base, Ohio, September 1959.

[**] K. H. Schaeffer and Albert Shapero, The Structuring and Analysis of Complex System Problems, Air Force Technical Note AFOSR 810, Stanford Research Institute, Menlo Park, California, under Contract AF 49(638)-1020 to Air Force Office of Scientific Research, May 1961.

In this connection, it is of interest to consider, briefly, the training of the knowledgeable systems analyst. This would involve instruction in the systems analysis procedures described. Standardized system development, evaluation, and compatibility situations would be used to train the analyst to identify the appropriate classes of discriminative stimuli, and to instruct him in the appropriate operations to be employed on those stimuli. A training regime designed to produce knowledgeable systems analysts can be constructed around a series of questions and operations selected to reflect the essential details of the successive validation procedure. This kind of training problem offers no special difficulties for the conventional lecture method, with appropriate visual aids. However, to obtain maximum, standardized effects, it is worthwhile to consider whether the design of a Systems Analysis Training Simulator is feasible. The contention here is that, using the operationally defined systems-analytic events already discussed, a Systems Analysis Training Simulator can be designed that will train the potential systems analyst to (1) phrase his systems problems in terms of generalized, operational concepts, (2) identify and specify the discriminative stimulus events upon which he should operate, (3) generate and validate the appropriate, successive sets of analytic events with respect to defining and constraining factors, (4) specify a prototype system configuration, (5) design procedures for evaluation, compatibility, and reliability testing, and (6) specify an operational system configuration.

Summary

The knowledgeable analyst is a discriminative responder. Using a stimulus-response discrimination model, we can describe and define operationally such analytic activities as observation, explanation, evaluation, problem-solving, and prediction.

A behavioral definition of meaning, necessary to understanding the activities of the knowledgeable analyst, is discussed in the stimulus-response context.

Operational procedures for systems analysis are presented with reference to problems of systems development, systems evaluation, and systems compatibility. These are based upon an operationally defined successive validation technique. Implications of this approach for training of the systems analyst are considered briefly.

VIII THE KNOWLEDGEABLE ANALYST AND THE
ANALYSIS OF COMPLEX SYSTEMS

The matrix-network approach presented in Chapter III is considered
to be an approach to systems analysis rather than a method of systems
analysis, since it lacks the precision required of a method. This lack
of precision expresses itself through the frequent referral to the knowl-
edgeable analyst's judgment as the decision criterion. In an effort to
illuminate the concept of the knowledgeable analyst, my co-authors discuss
various aspects related to this concept in Chapters IV through VII. Can
we now present on the basis of these insights a method for systems analy-
sis? If not, can we indicate what further work is required to sharpen
the approach into a method? Or, is an approach to systems analysis pref-
erable to a method for systems analysis?

To answer these questions let us consider first the conditions under
which one can avoid in an analysis the use of the knowledgeable analyst's
judgment as a decision criterion. In the preceding chapters we encountered
these conditions twice. First, in Chapter V, where we discussed the
linguist's position in phonetic analysis and noted that he was required
in this type of analysis only because at present "sufficient means for
the instrumentation of the sound-producing anatomical conditions" are
lacking, but in theory he is not needed because in theory one can define
the functions he performs with such rigor that if instrumentation were
available these functions could be instrumented. Second, in Chapter VII,
where Fink presents a description of the knowledgeable analyst without
reference to his judgment as an explanation of the systems analysis proc-
ess. Fink is able to do this by considering the analyst as a responder
to discriminatory observations, and by defining discrimination as "a
selective, differentiated association of events available to be responded
to and events that do the responding. For example, if an organism is
surrounded by a set of environmental events, call them E events: E_1,
E_2, E_3, . . . E_n, and if the organism has at his disposal a set of behav-
ioral events: B_1, B_2, B_3, . . . B_n, and if a subset of the available B
events occurs with a high degree of reliability to a subset of the E
events, we say that we are witnessing an instance of discrimination."
This definition of discrimination implies that the analyst is surrounded
by a finite number of environmental events, E, and has at his disposal a
finite number of behavioral events, B. Since however the E's and B's
tend even in the simplest situation to be astronomical in number (note

the discussion on phonemics in Chapter V) this definition maybe tells us what the analyst does, but it does not tell us how he does it, and it tells only what he does to the extent that both the observer of the analyst and the analyst are confronted by a limited number of E's and B's. The common element in both phonetics and Fink's discussion is the fact that they are restricted to a relationship of events for which, at least theoretically, rigorous analytical or probabilistic rules can be developed.

Conversely, we need the analyst's judgment when we lack such rigorous rules, or, in other words, when we lack full awareness of what the significant elements are and how the interrelationship between these elements can be defined. This concept appears in many different forms in the discussions on the knowledgeable analyst. For instance, in Chapter IV where Wainstein advocated the case study as a means to "help to ensure that the analyst will properly fit his analysis to the problem rather than the problem to the analysis" and as "a means of 'testing' the reality of the analyst's concepts, inputs, and conclusions"; in Chapter V where we showed that in phonemics the analyst is not precisely enough aware of what constitutes a significant sound group to develop rigorous rules for selecting them; and in Chapter VI where Rappaport after discussing purposive problem solving concludes that since "there may be more than one right path to reach many of the right answers that are possible, then, lacking omniscience, there is no alternative to developing enhanced thinking capability other than through testing and re-testing continually the world about us and the systems in it."

We can thus say that the analyst's judgment as a decision criterion is not needed if the elements affecting the analysis are so well defined that the analyst's capability to recognize and distinguish between them presents no problem. On the other hand, where the selection of those facts or elements that influence the purpose for which the analysis is performed present a major problem, that is, where the relevant facts or elements are not clearly defined or circumscribed and thus not unequivocally distinguishable from those of no consequence to the analysis, there the analyst's judgment must enter the analysis. The analyst's judgment is then the analyst's awareness of the facts relevant to the problem at hand. Or, the ploy of the knowledgeable analyst is introduced into the matrix-network approach wherever we require of the analyst an awareness of the problem at hand.

What is this awareness? It is a subjective, private state in the analyst's mind which leads him to the decision that certain elements and relations are or are not relevant to the analysis at hand. While this awareness can result in objectively observable operations it is not

identical with these operations nor deducible from these operations, except to the extent that it expresses itself through corresponding operations. Since awareness is purely subjective, it is itself not objectively verifiable.

Awareness is a well known concept of the Introspective school of psychology of the early twentieth century; the concept is, however, no longer part of modern experimental psychology. In introducing the concept here, are we not turning back the clock? Obviously, I do not think so, and I find my reasons in the purposes of experimental psychology and in the purpose for developing an approach for systems analysis. The experimental psychologist studies human behavior in order to predict it, and in his studies of human behavior he has found that he can predict behavior most unequivocally and parsimoniously if he restricts himself to terms that describe human behavior through objectively verifiable events. In other words, the experimental psychologist is a scientist who attempts to develop general theories for the prediction of human behavior by using a minimum of operationally well-defined terms and verifiable relations. However, in the matrix-network approach the primary interest is in developing an approach for analysts to follow in structuring complex systems, not in predicting their performance in structuring complex systems. We are thus not interested in predicting the analyst's behavior but rather in giving to other analysts concepts by which they can imitate the processes which have led problem solvers to structure complex systems successfully, and these processes include the problem solver's awareness of the problem.

The process of structuring complex systems is to a degree analogous to the driving of a car, and the driving of a car can be viewed from at least two different points of view. One is the point of view of an observing back-seat driver. He describes the driving of the car from the environmental factors he observes and the operations he sees the driver perform. Thus in speaking about driving he can say: "When a car approaches an intersection at which the traffic light is red the driver usually stops his car." Another way of looking at driving is from the driver's point of view, who might describe the same situations by saying: "When I see a red light at an intersection I stop my car." The former statement can be verified by other observers; the latter--since it is a report of the "I"--cannot be objectively verified. The former statement also indicates that sometimes the driver does not stop when the light is red, and that thus the relationship between "red light" and "stopping" is only a probable one. By contrast, the driver's statement is not a probable relationship, but is certain by definition, since it restricts itself to the individual's response to a situation of which he is aware.

129

If we attempt to tell someone what he must do to drive a car, it appears most appropriate to tell him that if he sees a red light at an intersection he should stop the car, rather than that a probable relationship exists between "red light" and "stopping." On the other hand, if we are interested in evaluating the driver's performance or predicting future performance, it is inconclusive to ask him if he stopped when the light was red, but through observation of objectively verifiable operations we must determine if and how frequently he actually stopped. Thus a practical approach to structuring complex systems which gives prospective analysts information on what they should and should not imitate must include the concept of awareness; however, an evaluation of the success with which the operation is performed, or, a prediction on how well certain operations will be performed does not need to contain the concept of awareness--in fact should not contain this concept since the concept is not objectively verifiable, and as experimental psychology has shown not necessary for the objective verification of operations.

Does the identification of the knowledgeable analyst's judgment with the analyst's awareness give us the clue we need for transforming the matrix-network approach into a method for systems analysis? Yes and no. A method is a rigorous, formal procedure in which each operation follows from the preceding operations, and such formal, objective procedures cannot entail subject concepts; thus they cannot entail awareness. Therefore, to elevate the approach to a method we must eliminate the need of "awareness" from the approach. As we noted, this can be done and can only be done if we are dealing with the structuring and analysis of well-defined problems. A method for systems analysis can thus be developed only if systems analysis is restricted to the analysis of well-defined problems.

If, however, the systems analyst is asked to analyze and structure solutions to natural problems which are ill-defined and ill-structured, and this is usually the case, he has a choice between three alternatives: (1) He can reject the problem as being too ill defined to be amenable to systems analysis; (2) He can restate the ill-structured problem intuitively in terms of a well-defined and structured problem and solve this problem by rigorous, formal methods; or (3) He can structure the nature problem through a systematic but informal approach which permits his awareness of the implications of the problem to influence each step of the analysis.

Since the first of these alternatives is a retreat from the problem we can reject it out of hand; however, we should not overlook, as we mentioned in Chapter II, that this path of retreat may be the prudent man's choice.

130

The advantages and dangers of the other two alternatives are obvious. In the second alternative formal, rigorous, and thus repeatable methods are used to obtain solutions of restated problems. The validity of these solutions in relation to the restated problem will be beyond doubt, but their meaningfulness to the natural problem may be only peripheral. If the natural problem is rather simple this is probably not the case (note the success of linear programming models for scheduling operations); but if the natural problem is complex, the one-time intuitive interpretation never checked, or checked only after the entire formal analysis is completed, may well lead an analyst astray. Still this is the alternative selected by those who feel that it is the systems analyst's job to solve problems solely with formal techniques, as for instance Bellman and Brock.* Note also that Fink, who avoids the use of the analyst's judgment as a decision criterion, must begin his "system development problem" (page 117) with explicitly stated mission objectives, environmental events, and performance requirements--in other words, with a well-defined rather than a natural problem.

In the third alternative, the analyst's awareness of the implications of the natural problem influences each step in the analysis, thus giving assurance, albeit informally, that he is solving the actual problem. Unfortunately, the informality of the various steps of the approach makes the precise repetition of these steps by other analysts doubtful. Still, this alternative appears to be the only way in which a solution to the real problem can be achieved with at least some degree of confidence, even if this degree of confidence cannot be formally measured.

In the matrix-netowrk approach I hope to have presented one way for giving us some confidence in the completeness of the analysis, and some confidence in its rigor through the use of mathematical models as systems descriptions wherever justifiable. However, the function that the mathematical models perform in the matrix-network approach differs from those they perform in the second alternative, where they are used to present a solution to the problem, whereas in the matrix-network approach they are used to highlight complex relationships to increase the analyst's (and ultimately the decision maker's) awareness of what is relevant to and implied by a solution of the natural problem. Observations of systems analysis in the actual decision-making processes suggest that mathematical models have in practice been used only in the latter way, even if those who performed the mathematical analyses thought they were used or should be used in the former. An approach to systems analysis involving a

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* Richard Bellman and Paul Brock, "On the Concept of a Problem and Problem Solving," American Mathematical Monthly, Vol. 67, No. 2, February 1960.

knowledgeable analyst can reflect better the questions that arise in the actual decision-making process, while a method for systems analysis can result only in a background study, which then still needs to be knitted, somehow, into the logical process of the actual decision-maker.

The concept that a knowledgeable man aided by rational approaches is needed for decision-making is not a new one. This concept is at least as old as Western man's writings. Plato's philosopher king, the stories of the line and of the cave, Kant's Antinomies, and by indirection Goedel's incompleteness proof are examples of this. While man's attempts to develop methods that explain everything in a single rational model have often effectively highlighted limited problems, these methods--from Democritus to Freud and Pavlov--when carried to their limits lead to meaningless tautologies. Likewise, man's attempt to dispense with pre-conceived notions and models and rely solely on experience has brought him no further than Hume's ludicrous identification of causation with habit. Man advanced in science as well as technology when he combined his capability for rational thought with his ever-growing awareness-- insight, intuition, call it what you will--of the problems surrounding him. While he can at times use methods to verify what he proposes, only an approach can lead him toward making good proposals, good decisions. It is my hope that our approach of structured thought and uninhibited judgment will provide a clue for those who seek to answer predesignated problems about complex systems.

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